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NEW CONSTRUCTIONS IN GRAPHICAL STATICS.

By HENRY T. EDDY, C. E., Ph. D., University of Cincinnati.

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VIII.

SPHERICAL DOME OF METAL.

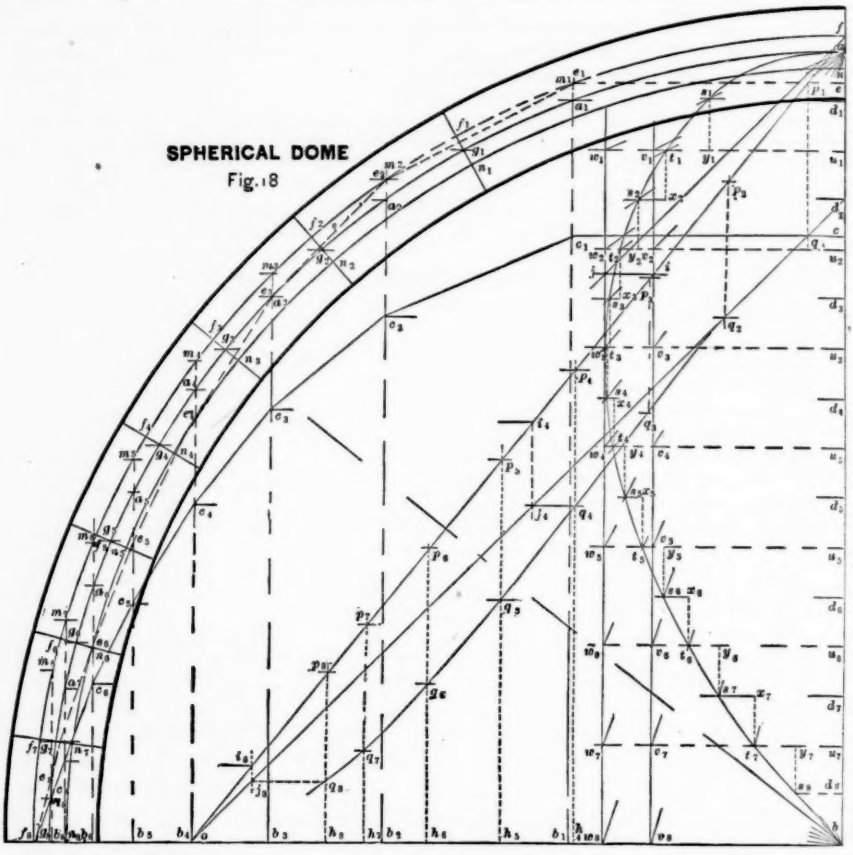
The dome which will be treated in the following construction is hemispherical in shape; but the proposed construction applies equally to domes of any different form generated by the revolution of the arc of some curve about a vertical axis: such forms are elliptic, parabolic or hyperbolic domes, as well as pointed or gothic domes, etc. Let the quadrant aa in Fig. 18, represent the part of the meridian section of a thin metallic dome between the crown and the springing circle. The metallic dome is supposed to be so thin that its thickness need not be represented in the Figure: the thickness of a dome of masonry, however, is a matter of prime importance and will be treated subsequently.

In a thin metallic dome the only thrust along a meridian section is necessarily in a direction tangent to that section at each point of it. This consideration will enable us to determine this thrust as well as the hoop tension or compression along any of the conical rings into which the dome may be supposed to be divided by a series of horizontal planes.

Let the height ab of the dome be divided into any number of parts, which we have in this case, for convenience, made equal. Let these equal parts of the type du be the distances between horizontal planes such that the planes through the points d_1, d_2, etc., cut small circles from the hemisphere which pass through the point a_1, a_2, etc., and similarly the planes through u_1, u_2, etc., cut small circles which pass through g_1, g_2, etc. Now suppose the thickness of this dome to be uniform, and if ab be taken to represent the weight of a quadrantal lune of the dome included between two meridian planes making some small angle with each other; then from the well-known expression for the area of the zone of a sphere it appears that ad_1 will represent the weight of that part of the lune above a_1d_1. Similarly au_1 is the weight of the lune ag_1; ad_2 the weight of aa_2, etc.

This method of obtaining the weight applies of course in case the dome is any segment of a sphere less than a hemisphere and of uniform thickness. If the thickness increases from the crown, the weights of the zones cut by equi-distant horizontal planes increase directly as the thickness. In case the dome is not spherical the weights must be determined by some process suited to the form of the dome and its variation in thickness.

Now the weight of the lune aa_1 is sus-



SPHERICAL DOME
Fig. 18

tained by a horizontal thrust which is the resultant of the horizontal pressures in the meridian planes by which it is bounded, and by a thrust, as before remarked, in the direction of the tangent at a. Draw a horizontal line through d_1, and through a a parallel to the tangent at a: these intersect at s_1, then is ad_1s_1 the triangle of forces which hold in equilibrium the lune aa_1. Similarly, au_1t_1 is the triangle of forces holding the lune ag_1 in equilibrium, etc. Draw a curve st through the points thus determined. This curve is a well-known cubic which when referred to ba as the axis of x and bg_8 as that of y has for its equation

$$\frac{y^2}{x^2}=\frac{r-x}{r+x}$$

On being traced at the right of a it has in the other quadrant of the dome a part like that here drawn forming a loop; it passes through b at an inclination of 45° and the two branches below b finally become tangent to a horizontal line drawn tangent to the circle aa of the dome. The curve has this remarkable property:—If any line be drawn from a, cutting the curve here drawn and, also, the part below bg_8, the product of these two radii vectores of the curve from the pole a is constant, and the locus of the intersection of the normals at these two points is a parabola.

Draw a vertical tangent to this curve: the point of contact is very near t_3, and g_3, the corresponding point of the dome is almost 52° from the crown a. A determination of this maximum point by means of the equation gives the height of it

above b as $\frac{1}{2}(\sqrt{5}-1)\,r$, corresponding to about 51°49′. Now consider any zone, as, for example, that whose meridian section is g_1a_2: the upper edge is subjected to a thrust whose radial horizontal component is proportional to u_1t_1, while the horizontal thrust against its lower edge is proportional to d_2s_2, and the difference s_2x_2 between these radial forces produces a hoop compression around the zone proportional to s_2x_2. It will be seen that these differences which are of the type sx or ty, change sign at t_3. Hence all parts of the dome above 51° 49′ from the crown, are subjected to a hoop compression which vanishes at that distance from a, while all parts of the dome below this are subjected to hoop tension. This may be stated by saying that a thin dome of masonry would be stable under hoop compression as far as 51° 49′ from the crown, but unstable below that, being liable to crack open along its meridian sections. A thick dome of masonry, however, does not have the resultant thrust at every point of its meridian section in a direction which is tangential to its surface,—this will be discussed later.

It is necessary to determine the actual hoop tension or compression in any ring in order to determine the thickness of the dome such that the metal may not be subjected to too severe a stress.

The rule for obtaining hoop tension (we shall use the word tension to include both tension and compression) is: Multiply the intensity of the radial pressure by the radius of the hoop, the product is the tension at any meridian section of the hoop. The correctness of this rule appears at once from consideration of fluid pressure in a tube, in which it is seen that the tensions at the two extremities of a diameter prevent the total pressure on that diameter from tearing the tube asunder.

Now in the case before us t_1y_1 is the radial force distributed along a certain lune. The number of degrees of which the lune consists is at present undetermined: let it be determined on the supposition that it shall be such a number of degrees as to cause that the total radial force against it shall be equal to the hoop tension. Call the total radial force P and the hoop tension T, then the lune is to be such that $P=T$. Also let θ be the number of degrees in the lune, then $90°\div\theta$ is the number of lunes in a quarter of the dome, and $90\,P\div\theta$ is the radial force against a quarter of the dome, which last must be divided by $\frac{1}{2}\pi$ to obtain the hoop tension; because if p is the intensity of radial pressure, $\frac{1}{2}\pi rp$ is the total pressure against a quadrant and rp, as previously stated, is the hoop tension. The ratio of these is $\frac{1}{2}\pi$, and by this we must divide the total radial pressure in every case to obtain hoop tension

$$\therefore\frac{180\,P}{\theta\pi}=T,\quad\therefore\ \theta=\frac{180°}{\pi}$$

$$\text{for } P=T\quad\therefore\ \theta=57°.3-$$

This is the number of degrees of which the lune must consist in order that when ab represents its weight, t_1y_1 shall represent the hoop tension in the meridian section a_1g_1. The expression we have found is independent of the radius of the ring, and hence holds for any other ring as g_1a_2, in which s_2x_2 is the hoop tension, etc. To find what fraction this lune is of the whole dome, divide θ by 360°

$$\therefore\frac{\theta}{360}=\frac{180}{360\pi}=\frac{1}{2\pi}=\frac{4}{25}\text{nearly},$$

from which the scale of weight is easily found, thus; let W be the total weight of the dome and r its radius, then

$2\pi r : W :: 1 : n$, the weight per unit, or the hoop tension per unit of the distances ty or sx.

Distances at or as, on the same scale, represent the thrust tangential to the dome in the direction of the meridian sections, and uniformly distributed over an arc of 57°.3− : *e.g.* if we divide at_3 measured as a force by $\theta\times u_2g_2$ measured as a distance we shall obtain the intensity of the meridian compression at the joint cut from the dome by the horizontal plane through a_2.

Analogous constructions hold for domes not spherical and not of uniform thickness. Approximate results may be obtained by assuming a spherical dome, or a series of spherical zones approximating in shape to the form which it is desired to treat.

SPHERICAL DOME OF MASONRY.

Let the dome treated be that in Fig. 18 in which the uniform thickness of the masonry is one-sixteenth of the internal diameter or one-eighth of the radius of

the intrados. Divide ab the radius of the center line into any convenient number of equal parts, say eight, at u_1, u_2, etc.: a much larger number would be preferable in actual construction. At the points a_1, a_2, etc., on the same levels with u_1, u_2, etc. pass conical joints normal to the dome, so that b is the vertex of each of the cones.

If we consider a lune between meridian planes making a small angle with each other, the center of gravity of the parts of the lune between the conical joints lie at g_1, g_2, etc. on the horizontal midway between the previous horizontals. These points are not exactly upon the central line aa, but if the number of horizontals is large, the difference is inappreciable. We assume them upon aa. That they fall upon the horizontals through d_1, d_2, etc., midway between those through u_1, u_2, etc., is a consequence of the equality in area between spherical zones of the same height.

In finding the volume of a sphere it may be considered that we take the sum of a series of elementary cones whose bases form the surface of the sphere, and whose height is the radius. Hence, if any equal portions of the surface of a sphere be taken and sectorial solids be formed on them as bases and having their vertices at the center, then the sectorial solids have equal volumes. The lunes of which we treat are equal fractions of such equal solids.

Draw the verticals of the type bg through the centers of gravity g_1, g_2, etc. The weights applied at these points are equal and may be represented by au_1, $u_1u_2=w_1w_2$, etc. Use a as the pole and w_1w_2 as the weight line; and, beginning at the point f_8, draw the equilibrium polygon c due to the weights.

We have used for pole distance the greatest horizontal thrust which it is possible for any segment of the dome to exert upon the part below it, when the hoop compression extends to 51° 49′ from the crown.

Below the point where the compression vanishes we shall not assume that the bond of the masonry is such that it can resist the hoop tension which is developed. The upper part of the dome will be then carried by the parts of the lunes below this point by their united action as a series of masonry arches standing side by side.

Now it is seen that the curve of equilibrium c, drawn with this assumed horizontal thrust falls within the curve of the lune, which signifies that the dome will not exert so great a thrust as that assumed. By the principle of least resistance, no greater horizontal thrust will be called into action than is necessary to cause the dome to stand, if stability is possible. If a less thrust than that just employed be all that is developed in the dome, then the point where the hoop compression vanishes is not so far as 51° 49′ from the crown, and a longer portion of the lune acts as an arch, than has been supposed by previous writers on this subject,* none of whom, so far as known, have given a correct process for the solution of the problem, although the results arrived at have been somewhat approximately correct.

To ensure stability, the equilibrium curve must be inscribed within the inner third of that part of the meridian section of the lune which is to act as an arch; as appears from the same reasons which were stated in connection with arches of masonry.

And, further, the hoop compression will vanish at that level of the dome where the equilibrium curve, in departing from the crown, first becomes more nearly vertical than the tangent of the meridian section; for above that point the greatest thrust that the dome can exert, cannot be so great as at this point where the thrust of the arch-lune is equal to that of the dome.

Now to determine in what ratio the ordinates of the curve c must be elongated to give those of the curve e which fulfills the required conditions, we draw the line fo, and cut it at p_1, p_2, etc. by the horizontals m_1p_1, m_2p_2, etc., the quantities mb being the ordinates of exterior of the inner third. Again draw verticals through p_1, p_2, etc., and cut them at q_1, q_2, q_3, etc. by horizontals through c_1, c_2, c_3, etc. Through these points draw the curve qq, whose ordinates are of the type qh. Some one of these ordinates is to be elongated to its corresponding ph, and in such a manner that no qh shall

* See a paper read before the Royal Inst. of British Architects, "on the Mathematical Theory of Domes," Feb. 6th, 1871. By Edmund Beckett Denison, L.L.D., Q.C., F.R.A.S.

then become longer than its corresponding ph. To effect this, draw oq_2 tangent to the curve qq; then will oq_2 enable us to effect the required elongation: *e.g.* let the horizontal through e_4 cut oq_2 at j_4, and then the vertical through j_4 cuts fo at i_4, then is e_4 (which is on the same level with i_4) the new position of c_4. Similarly, we may find the remaining points of the curve e; but it is better to determine the new pole distance, and use this method as a test only.

The curve qq made use of in this construction for finding the ratio lines for so elongating the ordinates of the curve c, that the new ordinates shall be those of a curve e tangent to the exterior line of the inner third, may be applied with equal facility to the construction for the arch of masonry. This furnishes us with a direct method in place of the tentative one employed in connection with Fig. 14.

To find the new pole distance, draw $ff \parallel oq_2$ cutting ww at j, then will i the intersection of the horizontal through j, be the new position of the weight line vv, having its pole distance from a diminished in the required ratio.

The equilibrium curve e will be parallel to the curve of the dome at the points where the new weight line vv cuts the curve st. It should be noticed that the pole distance which we have now determined is still a little too large because the polygon e is circumscribed about the true equilibrium curve; and as the polygon has an angle in the limiting curve mm the equilibrium curve is not yet high enough to be tangent to the limiting curve. If the number of divisions had originally been larger (which the size of our Figure did not permit) this matter would be rectified.

The polygon e is seen at e_5 to fall just without the required limits, this would be partly rectified by slightly decreasing the pole distance as just suggested; the point, however, would still remain just without the limit after the pole distance is decreased, and by so much is the dome unstable. A dome of which the thickness is one fifteenth of the internal diameter, is almost exactly stable.

It is a remarkable fact that a semicylindrical arch of uniform thickness and without surcharge must be almost exactly three times as thick, viz., the thickness must be about one fifth the span in order that it may be possible to inscribe the equilibrium curve within the inner third.

The only large hemispherical dome, of which I have the dimensions, which is thick enough to be perfectly stable without extraneous aid such as hoops or ties, is the Gol Goomuz at Beejapore, India. It has an internal diameter of 137½ feet, and a thickness of 10 feet, it being slightly thicker than necessary, but it probably carries a load upon the crown which requires the additional thickness.

The hemispherical dome of uniform thickness is a very faulty arrangement of material. It is only necessary to make the dome so light and thin for 51° 49′ from the crown that it cannot exert so great a horizontal thrust as do the thicker lunes below, to take complete advantage of the real strength of this form of structure. A dome whose thickness gradually decreases toward the crown takes a partial advantage of this, but nothing short of a quite sudden change near this point appears to be completely effective.

The necessary thickness to withstand the hoop compression and the meridian thrust can be found as previously shown in the dome of metal.

Domes are usually crowned with a lantern or pinnacle, whose weight must be first laid off below the pole a after having been reduced to the same unit as that of the zones of the dome.

Likewise when there is an eye, at the crown or below, the weight of the material necessary to fill the eye must be subtracted, so that a is then to be placed below its present position. The construction is then to be completed in the same manner as in Fig, 18.

It is at once seen that the effect of an additional weight, as of a lantern, at the crown, since it moves the point a upward a certain distance, will be to cause the curve st to have all its points except b to the left of their present position, and especially the points in the upper part of the curve, thus making the point of no hoop tension much nearer the crown than in the metallic dome. It will be noticed that the addition of very small weight at the crown will cause the point m_2 of no hoop tension in the dome of masonry to approach almost to the crown, so that then the lunes will act entirely as stone

arches with the exception of a very small segment at the crown.

On the contrary, the removal of a segment at the crown, or the decrease of the thickness, or any device for making the upper part of the dome lighter will remove the point of no hoop tension further from the crown, both for the dome of metal and of masonry. In any dome of masonry the thickness above the point of no hoop tension, as determined by the curve *st*, need be only such as to withstand the two compressions to which it is subjected, viz; hoop compression and meridian compression: while below that the lunes acting as arches must be thick enough to cause a horizontal thrust equal to the maximum radial thrust of the dome above the point of no hoop tension.

Several large domes are constructed of more than one shell, to give increased security to the tall lanterns surmounting them: St. Peter's, at Rome, is double, and the Pantheon, at Paris, is triple. The different shells should all spring from the same thick zone below the point of no hoop tension; and the lunes of this thick zone should be able to afford a horizontal thrust equal to the sum of the radial thrusts of all the shells standing upon it.

Attention to this will secure the stability in itself of any dome of masonry spherical or otherwise; and, though I here offer no proof of the assertion, I am led to believe that this is the solution of the problem of constructing the dome of a minimum weight of material, on the supposition that the meridian joints can afford no resistance to hoop tension.

Now, in fact, it is a common device to ensure the stability of large domes by encircling them with iron hoops or chains, or by embedding ties in the masonry; and this case appears to be of sufficient importance to demand our attention.

If the hoop encircles the dome at 51° 49′ or any other less distance from the crown the dome will be a true dome at all points above the hoop. Suppose the hoop to be at 51° 49′, then the curve *e* should, below that point, be made to pass through the points f_3 and f_8, from which it is seen that the dome may be made thinner than at present, and the horizontal thrust caused will be less. The tension of the hoop would be that due to a radial thrust which is the difference between that given by the curve *st* for this point and the horizontal thrust (pole distance) of the polygon *e* when it passes through f_3 and f_8. That the curve *e* passes through these last mentioned points is a consequence of the principle of least resistance.

Again, suppose another hoop encircles the dome at f_5; the curve *e* must pass through f_5 and f_9, and in this part of the lune will have a corresponding horizontal thrust. The curve *e* must also pass through f_6 and f_8, but in this part of the lune will have a horizontal thrust corresponding to it, differing from that in the part between f_5 and f_6: indeed the horizontal thrust in the segment of a dome above any hoop depends exclusively upon that segment and and is unaffected by the zone below the hoop. The tension sustained by the hoop is, however, due to the radial force, which is the difference of the horizontal thrusts of the zones above and below the hoop.

It is seen that the introduction of a second hoop will still further diminish the thickness of lune necessary to sustain the dome, unless indeed the thickness is required to sustain the meridian compression.

Had a single hoop been introduced at f_6 with none above that point, the dome above f_6 should then be investigated, just as if the springing circle was situated at that point. The curve *e* must then start from f_6, as it before did from f_8, and be made to become tangent to the limiting curve at some point between f_5 and the crown.

By the method here employed for finding the tension of a hoop it is possible to discuss at once the stresses induced in the important modern domes constructed with rings and ribs of metal and having the intermediate panels closed with glass.

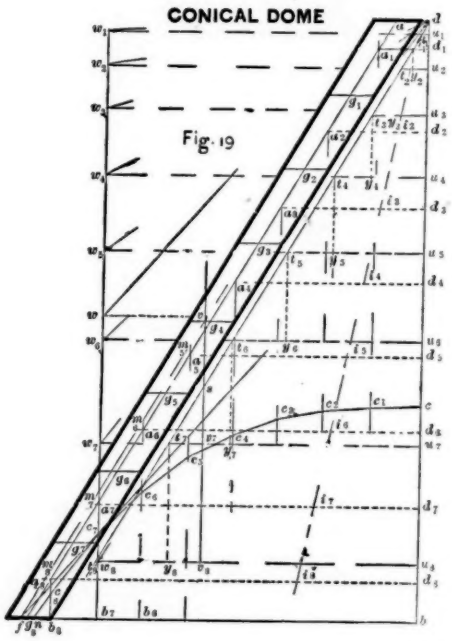
On introducing a large number of rings at small distances from each other, it will be seen that the discussion just given leads to the method previously given for the dome of metal.

The dome of St. Paul's, London, is one which has excited much adverse criticism by reason of the novel means employed to overcome the difficulties inherent in so large a dome at so great a height above

the foundations of the building. The exterior dome consists of a framework of oak sustained by conical dome of brick which forms the core. There is also a parabolic brick dome under the cone which forms no essential part of the system. Since the conical dome in general presents some peculiarities worthy of notice we will give an investigation of that form of structure as our concluding construction.

CONICAL DOME OF METAL.

In Fig. 19, let bd be the axis of the frustum of a metallic cone cut by a vertical plane in the meridian section a. The cone is supposed to have a uniform thickness too small to be regarded in comparison with its other dimensions. Suppose the frustum to be cut by a series of equi-distant horizontal planes as at g_1, g_2, etc., into a series of frustra or rings: then the weight of each ring is proportional to its convex surface. The convex surface of any ring $=2\pi r\times$ slant height; when r is half the sum of the radii of the two bases, *i.e.*, r is the mean radius. Consequently, the weights of these rings, or any given fraction of them included between two meridian planes, is proportional to their mean radii. Let us draw these mean radii d_1a_1, d_2a_2, etc., be-



tween the horizontals through g_1g_2, etc., and use some convenient fraction, say $\frac{1}{3}$, of these quantities of the type du as the weights. The line ii cuts off $\frac{1}{3}$ of each of these: then lay off $du_1=d_1i_1$ as the weight of the ring ag_1, lay off $u_1u_2=d_2i_2$, $u_2u_3=d_3i_3$, etc., as the weights of the rings g_1g_2, g_2g_3, etc.

Draw the line $dt \parallel aa$, it corresponds to the curve st of Fig. 18; then the quantities of the type tu represent the horizontal radial thrust which the cone exerts upon the part below it, while the radial thrust borne by any ring is the difference between two successive quantities of the type tu, *i.e.*, the radial thrust in the ring g_1g_6 is represented by t_2y_6, that in g_6g_7 by t_7y_7, etc. As previously shown in connection with the spherical dome, if the scale of weights be such that du_3 represents a part of the cone between two meridian planes which make an angle of $\theta=180°\div\pi=57°.3$—, then will t_3y_3, t_7y_7 etc., be the total hoop com-

pression of the corresponding rings of the cone. It is to be noticed that this quantity does not change sign in the cone, and is always compression.

The meridian compression is expressed, under the same circumstances by the quantities dt_s, dt_1, etc.

Such a cone as this must be placed upon a cylindrical drum or other support which can exert a resistance in the direction aa, but if this support is very slightly displaced by the horizontal radial thrust, a hoop tension will be induced at the base of the cone. As this displacement is very likely to occur it is far better to have the base of the cone sufficiently strong to withstand this tension, which is $t_s u_s$ when du_s is the weight of 57°.3 : then the supports will sustain a vertical force alone.

This discussion applies equally well to a cone formed of a network of rings and inclined posts with intermediate panels of glass or other material.

CONICAL DOME OF MASONRY.

Let us assume that the uniform horizontal thickness of the dome to be treated, is one sixteenth of the internal diameter of the base, or one eighth of the internal radius, as shown in Fig. 19. The actual thickness is less than this, but since the horizontal thickness is a convenient quantity, we shall call it the thickness unless otherwise specified.

Pass equidistant horizontal planes as previously stated: then the volumes of these rings may be found by the prismoidal formula. The volume

$$=\tfrac{1}{6}\pi h\,[r_1^2-r_1'^2+4(r^2-r'^2)+r_2^2-r_2'^2],$$

in which h is the height of the ring, r_1 and r_1' are the radii external and internal of one base, r_2 and r_2' of the other, and r and r' of the middle section. Now $r_1-r_1'=r-r'=r_2-r_2'=t$ the thickness of the cone; and

$$r_1+r_2=2r,\ r_1'+r_2'=2r'$$
$$\therefore \text{Volume}=\pi ht(r+r')=2\pi ht\bar{r}$$

when $\frac{1}{2}(r+r')=\bar{r}$ the mean radius of the middle section. From this it is seen that the weights vary in the same manner, and are represented by the same quantities as previously stated in case of a thin cone. Assume that the centers of gravity of any thin lunes cut from these rings by meridian planes making a small angle with each other, are at the middle points a_s, a_1, etc., this assumption is sufficiently exact for the part of the cone near the base, which we are now specially to investigate.

By means of the weights $w_s w_1=u_s u_1$, etc., at some assumed distance from the pole d, describe the equilibrium polygon c, starting from n at the inner third of the base.

Now if the cone stands upon a drum which necessarily exerts a sufficient radial thrust to keep the meridian joints of the cone closed down to the base, then all the circumstances will be precisely as before explained in respect to the metallic dome: but if the drum exerts a less radial thrust, the meridian joints will open near the base, and the conditions of stability of that part of the cone will need to be investigated, as was done in the spherical dome of masonry, by considering the upper part of the dome as sustained by a series of stone arches. From f draw fc_1 tangent to the curve c; then must $c_1 b_1$ be elongated to $m_1 b_1$ and the other ordinates of c must be elongated in the same ratio in order that the equilibrium polygon may be tangent to the exterior limit fm; and, further, fm and fc_1 are the ratio lines by which to effect the elongation. To find how much the thrust is diminished, draw through the intersection of fm with bd, a line parallel to fc_1 intersecting the weight line at w, and then v the point where the horizontal through w intersects fm gives us the new position of the weight line, and its distance from the pole d. This vertical intersects tt about midway between t_s and t_1, thus showing that the meridian joints of the cone will be open from the base to about the point g_s. It is unnecessary to draw the equilibrium polygon in its new position.

We thus obtain the least horizontal thrust against which the dome can stand. The actual thrust which the drum exerts may have any value greater than this least thrust.

It is seen that the effect of diminishing the thickness of the cone, is to carry the tangent point c_1 and the point of no compression nearer to the base. In other words the thin dome of masonry of given semi-vertical angle necessarily exerts a greater thrust in proportion to its weight than does a thick dome, though that pro-

portion is unchanged if the joints are to remain closed all the way to the base.

All of the circumstances respecting radial thrust above the point of no hoop compression, and respecting meridian thrust, are the same as in the metallic cone.

Any additional loading above that of the weight of the cone itself, as for example, the weight of a lantern, or of an external dome, as in the case of St. Paul's, can be introduced and treated as an additional height or thickness of certain rings of the cone. The same method which has been here applied may be applied to all such cases, if the weights be determined by some suitable process. For example, it may be shown by the help of the prismoidal formula, that the volume of the ring cut from a uniformly tapering cone by equidistant horizontal planes, varies as the product of the mean radius of the mid-section by the thickness at the mid-section.

OTHER VAULTED STRUCTURES.

Similar principles to those above developed apply to domes with an elliptical or polygonal base, to domes whose meridian sections are ogee curves, to Skew Arches, to Groined Arches formed by the combination of cylindrical arches, as well as to Groined Arches which are dome-shaped.

By the application of the principles developed it is easy to treat the cone or dome which sustains the pressure of earth or water. Indeed, it is not too much to say that the complete solution of the problem of the stability of vaulted structures has now been set forth for the first time, and that the proper connection and relationship between similar structures, in metal and masonry, may now be clearly seen. In particular, the discussions have made manifest the applicability of a particular equilibrium polygon among the infinite number which are due to a given set of weights, and which are all projections of any one of them, and the possibility of deriving from it in each of the structures treated, a complete and sufficiently exact solution.

CONCERNING THE ST. LOUIS ARCH.

By C. SHALER SMITH, C.E.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the sixth number of Prof. Eddy's series of articles on "New Constructions in Graphical Statics" it is stated, that—"the St. Louis Arch is wanting in initial stiffness to such an extent, that the weight of a single person is sufficient to cause a considerable tremor over an entire span."

This belief, which the writer has found quite common in the East is erroneous in the highest degree. Light teams habitually trot at a good rate of speed over these arches without producing a tremor. Railway trains have less effect than on a stiff truss bridge: military and other processions will produce a tremulous wave not exceeding the motion caused by the same agency on such a span as that in the Newport Bridge at Cincinnati, while the maximum effect of all is produced by the passage of a coal wagon loaded to seven tons, jolting over the rough flooring. This tremor exceeds any that the writer has observed on other structures, excepting the suspension bridges, but as the wave motions produced by jarring loads increase with the length of span, it is very clear that *until a beam truss span of 520 feet is built somewhere for highway purposes, we will have no opportunity of making a valid comparison.*

Theorising on this special subject is useless, as the nearest approach to the peculiar tremor alluded to, is to be found in the case of the 210 feet span *plate girder* over Mill Creek in Cincinnati, in which bridge the floorway is carried through on the line of the neutral axis. (On this point see Du Bois' Graphical Statics, second edition, page 363, paragraph 12).

Prof. Eddy's opinion as to the greatest economy of the flexible arch with con-

tinuous stiffening girder would probably be changed in case he were called on to design and proportion the actual structures at 500 feet span, and 50 feet rise. The writer made an exhaustive comparison between these two forms of arch (flexible and braced ribs) in 1870, at the request of Mr. Wm. MacPherson, then President of the St. Louis Bridge Co., and found that, contrary to his previously expressed belief, there was no practical difference between them in weight, while as was subsequently proved, the fact that the braced arch *could* be erected without false work, while the flexible arch could not, placed the latter far in the rear in actual economy of construction for that particular place. The conclusion concerning the St. Louis Arch arrived at by Prof. Du Bois, see pages 407 and 408 Graph. Statics, second edition, is more nearly correct than Prof. Eddy's belief "that the stiff arch rib was a costly mistake," or than Capt. Eads' own opinion as to the relative economy of the braced continuous rib as compared to the rib with hinges.

THE SEWAGE QUESTION.

By C. NORMAN BAZALGETTE, Barrister-at-Law.

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I.

The object of this Paper is twofold. First, to limit and define the proper application of the various systems which have from time to time been introduced, as a more or less efficient means of dealing with the sewage of towns. Secondly, to direct attention to certain subordinate questions arising upon the practical operation of these systems.

The Sewage Question has not made much progress, so far as discovery and invention are concerned, during the last quarter of a century, nor does there seem to be much prospect of any new or startling light being thrown upon it in the future. The fact is the field of investigation is nearly exhausted. Earth, air, fire and water have all been resorted to, and the relative merits of processes based upon them are pretty well known, while the resources of chemistry have been ransacked in turn. And yet even now many sanitary authorities stand in an attitude of doubt and expectation, as though, distrustful of the present, they waited for the advent of some miraculous invention which was to supplant all previous doctrine and set the whole question at rest. The causes of such hesitation are doubtless, first, the introduction by empirics of numerous processes which, professing to deal with sewage at a profit, have little or no real value. Secondly, the division among experts themselves, arising from the tendency to study and elaborate special crotchets to the exclusion of a comprehensive form of practice, adapted to the varying conditions and necessities of different localities. Thirdly, the proneness of every inventor to overrate the merits and exaggerate the capacity of his own invention. The most recent and conspicuous illustration of this latter defect may be found in the pretensions of Intermittent Downward Filtration, as put forward by the Rivers Pollution Commissioners.

The following classification of the various systems has been adopted for the purposes of this Paper :

I. Treatment with chemicals.
II. Application of sewage to land, including irrigation and intermittent downward filtration.
III. The dry earth system.
IV. The Liernur or pneumatic system.
V. Seaboard and tidal outfalls.

I. TREATMENT WITH CHEMICALS.

This category may be taken to include all those multifarious processes which, by the admixture of chemicals, have sought the purification of sewage by the precipitation of the dissolved and suspended impurities it contains, and the ultimate realization of the precipitate in

the form of manure. It would be beyond the province of this Paper to attempt even the most meagre summary of the various methods which have been suggested for the accomplishment of these objects. More than a century had elapsed since the first experiments were instituted at Paris, with a view to the extraction of the manurial elements of sewage in a compact form; and since that time, more than sixty permutations and combinations of the original idea of treatment by chemicals have seen the light. But although minute criticism of the comparative merits is impossible, it will still be necessary, for the purpose of defining their appropriate application, that some reference should be made to a few of the more conspicuous modern processes.

Before proceeding to their separate consideration, it may be stated that no chemical process can, unaided, effectually deal with sewage, but that its application must be ancillary to some other process, as, for instance, the disposal of sewage upon land. This partial incapacity of chemical reagents is attributable to the well-established fact that, in the adoption of any process, one of two results is inevitable; either purification must be purchased at the cost of insolvency, or solvency must be preserved at the sacrifice of purification; in other words, any chemical process to effect purification must be worked at a loss. For, conceding that even some of the agents employed can produce a fairly satisfactory effluent, there is no instance upon record where such a result has been obtained concurrently with a commercial profit, or even without a positive loss, although there is abundant evidence to the contrary. This failure must be mainly ascribed to the cost of the chemicals; for while to secure purification it is absolutely necessary that they should not be stinted, the only set off against their cost is the return derivable from the sale of the precipitate as manure. Experience has proved that sewage manures are unmarketable; and even assuming that this were not the fact, still, inasmuch as the value of the manure is pretty nearly in a direct ratio to the chemicals introduced into it, it is clear that, making allowance for waste, there must be an inevitable deficit. Apart, however, from this financial objection, there is a formidable practical difficulty attaching to the use of these processes, which up to the present time has proved an almost fatal bar to their efficient operation, namely, the difficulty of dealing with the enormous quantity of sludge developed by the employment of precipitants. From time to time a number of expedients have been suggested (the most recent by General Scott, R.E., Assoc. Inst. C.E.) to reduce this sludge into a manageable and inoffensive form. But hitherto the success attending the application of these expedients has been so partial, and the difficulty has been so insuperable, that in most cases, where treatment by chemicals has been or is practised, a limited manipulation only has been attempted, and the great bulk of the sewage residuum has been allowed to go to waste. Accordingly, even the doubtful return derivable from the sale of manure, when considered as recoupment for the cost of chemicals, must be subjected first of all to a liberal discount in respect of the difficulty of extracting it in a saleable form. Thus there are three urgent objections to the general adoption of any precipitating process as a means of dealing with sewage. First, it is so costly that purification can only be effected at a serious pecuniary loss; secondly, the extreme difficulty of manipulating the sludge and extracting the manure; and, lastly, the unmarketable character of the extract. A conviction of the truth of these conclusions is steadily forcing itself upon the public mind, and the various companies who are working chemical sewage patents begin to find bankruptcy as one alternative, or abandonment of the purification as the other.

As long ago as 1865, Mr. Rawlinson, C.B., M. Inst. C.E., in the Third Report of the Sewage of Towns Commission, epitomised the results gained up to that time, and stated that experience, so far as carried, proved that fluid sewage could not be manipulated into a solid manure so as to pay. It remains to be seen whether the results he cited have been substantially varied by subsequent practice.

1. *The Lime Process* may be considered as the progenitor of most chemical methods of treatment in this country,

having been adopted more than twenty years ago at Leicester. Its comparative efficiency is proved by the fact that its use survives in some of the more conspicuous instances of modern practice. It was included in the condemnation pronounced by Mr. Rawlinson in 1865; and the opinion then expressed has been confirmed by the Rivers Pollution Commissioners in their first report, 1870. They say, referring to the operation of the lime process at Leicester, Tottenham, and Blackburn, "In all these places the plan has been a conspicuous failure, whether as regards the manufacture of valuable manure, or the purification of the offensive liquid;" and again, "the method obviously failed in the purification of the sewage to such an extent as to render it admissible into a river." So far as Leicester is concerned, the accuracy of these statements has been verified by a personal inspection which the Author made of the works in the spring of 1876. The river Soar was then in a state of active pollution, although the most serious effect was observable at a point comparatively remote from the outfall works; but this apparent anomaly was easily accounted for by the fact, that a certain time must elapse before the effluent undergoes complete putrefaction, and it is thus enabled to flow some distance down the stream before arriving at its most offensive condition. About 300 tons of semi-solid sludge were being produced weekly, the estimated value of which was 6*d.* per ton, and yet even at this nominal price it could only command a market among the farmers to the extent of from 400 to 500 tons per annum, or something less than a fortnight's supply, while the remainder had to be got rid of upon 500 acres of drying ground, to the admitted nuisance of the adjacent cottages. The estimated cost for works incurred in respect of this process at Leicester, exclusive of sewerage works, is about £40,000, and the result is a complete failure, both as regards purification and the profitable extraction of a manurial compound. As Leicester is about to abandon the lime process for irrigation, it is unnecessary to dwell further upon the unsatisfactory effect of its adoption there.

At Birmingham the history of the lime process is perhaps more curious, and scarcely less instructive. Previous to 1871, the sewage there was allowed to subside in tanks, before being passed into the rivers Rea and Tame. In that year the Town Council, being alive to the defects of the system, and having an additional stimulus to action in a couple of injunctions obtained against them, appointed a committee to report upon the best means of dealing with the sewage of the town. This committee in the course of the same year presented a valuable and exhaustive report, in which, after describing the *modus operandi* of the lime process, they pass the following opinion upon its merits: "The deposit, in the form of a highly putrescible mud, is taken out, dried, and sold for manure; the process of drying being tedious and offensive, and in winter almost impossible. This process is the simplest and least costly of any, but according to the report of the Rivers Pollution Commissioners, the effluent water, though clarified, is not purified or fit to be admitted into a running stream, since it still contains about one-half of the putrescible organic matter, which is precisely what is most important to keep out. Moreover, as the effluent water still contains most of the valuable constituents, the manure is of little value, and it is difficult, and on a large scale might be impossible, to dispose of it."

Bearing in mind that this stricture on the lime process was pronounced as recently as 1871, it is noteworthy that this process has since been selected and applied by Mr. Hawksley, Past President Inst. C.E., for the purpose of dealing with the sewage of the town. Judging from the information obtained by a personal inspection of the works in the spring of 1876, the Town Council appear to have spared neither pains nor expense to verify the conclusions previously expressed by their Sewage Committee. The population contributing sewage is about 354,000; and, although it is not easy to ascertain the exact expenditure upon land and works at the outfall, yet, approximately, £38,000 must have been paid for 115½ acres of land, then there is the lease of 105 acres, while the cost of the old and new tanks may be calculated at £18,000 and £30,000 respectively, so that the outlay in round numbers has probably exceeded

£100,000. On arrival at the works the sewage is treated daily with about 15 tons of lime. It is next passed through eighteen depositing tanks, in which the accumulation of sewage residuum varies in amount and density in proportion to the distance of the tanks from the sewer outfall. The clarified effluent is then allowed to pass by various outlet sluices into the Rea or the Tame, or is disposed of by irrigation on the Corporation land. The amount of sludge produced daily is about 500 tons, an infinitesimal portion of which is converted into cement by General Scott's process. At the time of the Author's visit only three men were engaged in the manufacture of this cement. This will afford some criterion of the limited quantity of sludge dealt with. The residue of this enormous bulk of sewage matter, with the exception of a few boat loads, sold to the farmers at £1 a boat load, has to be dug into drying beds covering a large acreage, which is being continually extended. But although the operation of digging it into the ground is incessant, the accumulation of sludge is so enormous as to be almost unmanageable, even the large area at the disposal of the Corporation being insufficient for present requirements—the soil, in many parts to a considerable depth, being saturated and sodden with the impurities which it contains. Such a condition of things, in the neighborhood of a populous and important town, cannot be too seriously deprecated, both as tending to prejudice the health of the inhabitants and to depreciate the value of property. The machinery by which the process is applied is costly, ingenious, and elaborate, but the results from a sanitary point of view, except as a temporary expedient, can only be regarded as a calamitous failure.

The Corporation of Birmingham have, it is true, been exceptionally unfortunate in their efforts to deal with the sewage difficulty. In 1872, through an extraordinary exercise of land-owning influence, they lost a Bill, the object of which was an irrigation scheme, by a narrow majority upon third reading, after it had previously received the sanction of a Parliamentary Committee. It is to be hoped that the lime process, as practised at Birmingham, is merely a temporary means, pending the adoption of some more substantial and efficient mode, although the permanent and expensive character of the works rather tends to preclude so sanguine an expectation. But it is not only in the creation of this cumbersome mass of sewage mud that the process fails at Birmingham, for the effluent water, to reiterate the expression of the Sewage Committee, "though clarified, is not purified, or fit to be admitted into a running stream." Indeed, at the time of the Author's visit, some of the Corporation land was actually under irrigation by the effluent, and the attendant remarked naively enough, that for this purpose they preferred the effluent to the raw sewage. In that simple remark lay the confession of the practical inefficiency of the whole system; for it proved that the effluent still contained the putrescible organic matter which while the most fertilising is also the most actively polluting property of sewage, and which, therefore, should be most jealously excluded from the stream. It may perhaps be argued, that the effect of some of these chemical processes is not only to purify the sewage, but to give to the effluent water a manurial principle non-polluting in itself. This, however, is not the case, at all events with the lime process, for the fertilising power of the effluent is not due to any innocuous manurial principle which is added, but rather to the presence of the nitrogenous organic matter which it has failed to abstract. This view is confirmed by the Rivers Pollution Commissioners in their first report, where in speaking of the lime and Sillar's processes, they say: "The material, however, which it is of the greatest importance to remove from the dissolved constituents of sewage, is nitrogenous organic matter, because it is this kind of organic matter which enters rapidly into putrefaction, and becomes an active agent in the pollution of rivers. This material is represented in the analytical results by organic nitrogen. It is precisely here that both processes signally fail (although the lime process is slightly superior to the other) in accomplishing such an amount of purification as would render the sewage admissible into an open watercourse."

The lime process must therefore be taken to have failed at Birmingham, as

at Leicester, and if it has failed at Birmingham it will never succeed elsewhere, for all that experience can suggest or skill effect, in providing appropriate mechanical apparatus has been accomplished there. Yet it has failed in the first requirement of any process, namely, to produce a sufficient purification of the treated sewage. Moreover, an enormous amount of comparatively worthless sewage mud is being continually accumulated, the disposal of which is both difficult and offensive.

2. *The A B C, or Sillar's Process* (as worked by the Native Guano Company) claims a Mosaical origin, but it has been more sensational than successful in its public career.

The Rivers Pollution Commissioners, in their first report, placed the A B C process, as a purifier of sewage, upon a par with the lime process, but condemned both as failing to purify to such an extent as to render sewage admissible into running waters. In a subsequent Report of 1870, after detailing their investigations, extending over a period of nearly two years, the Commissioners present certain summarised conclusions which may be shortly paraphrased as follows —1. On no occasion has the purification been sufficiently complete to render the effluent admissible into running water : 2. The effluent is little better than what can be obtained by mere subsidence : 3. The manure has a low market value, and cannot repay the cost of production : 4. The manipulations incident to the process are nauseous and offensive.

These conclusions are based upon irrefragable evidence, which is set out in the Report. It may be instructive to extract an answer from the evidence of Dr. Odling, who was examined as an independent witness before the Commissioners, and whose evidence is appended to their Report: *Q.* 51, (by Dr. Frankland), "Looking at the whole result of the experiments, do you consider that this is a process to be recommended for the purification of town sewage?—*A.* Certainly not. No doubt this method, like all other methods of precipitation, does keep out a considerable proportion of filth from the river, but there was a great deal of putrescible matter in the effluent liquid, and in comparing this mode of precipitation with others, it did not seem to me that its alleged superiority had any foundation. As regards its superior defecation of sewage, and the high value of the product yielded, I came to the conclusion that it was simply a juggle."

To the sweeping condemnation expressed in this report, one invariable plea has been put forward by the promoters of the process, to the effect that, upon each occasion when the test experiments were made, there was some abnormal cause in operation, which unfortunately disturbed the accuracy of the results obtained, and therefore precluded their adoption as a basis upon which a judgment could be formed as to its general merits and capabilities. But the Commissioners, in the same Report, referring to the failure of the process, completely dispose of this allegation, for they say, "And if this failure be charged upon any alleged unfairness in the condition under which the experiments took place at Leicester and at Leamington, it must be pointed out in reply, that when the experiment was tried in the laboratory under circumstances which excluded all possible sources of error, the same result was still more unmistakable."

But a still more practical answer was soon afforded. On the 29th of January, 1871, the sanction of the Metropolitan Board of Works was obtained by the Native Guano Company to a proposal that experiments should be instituted at Crossness, in order to illustrate the efficacy of the A B C process. On the 11th of January 1873, or two years subsequently, Sir Joseph Bazalgette, C.B., M. Inst. C.E., the Engineer, and Mr. Keates the Chemist, of the Metropolitan Board, respectively, presented reports upon the results observed and recorded during that period, the gist of which may be shortly stated. The Engineer reported that the cost of producing a manure amounted to £6 6*s.* 4*d.* per ton. The Chemist put the net value of the manure at 20*s.* a ton, but reported "That the effluent water was, on the whole, very good. The A B C treatment so far clarified and defecated the sewage that, looking solely to the physical condition and chemical composition of the water produced at Crossness, I am of opinion that such water was in a fit state to be ad-

mitted into any ordinary river without producing a dangerous degree of pollution. I must here again direct attention, however, to the extremely dilute state of the sewage during the experiment. The effect of this was to render the results somewhat inconclusive, as it is, of course, impossible to say, at least from this experiment, how far the A B C treatment would defecate sewage of a stronger character." So that, in fact, such a "very good" effluent was obtained from "extremely dilute" sewage at a dead loss of £5 6*s.* 4*d.* per ton of manure, or at such a price as effectually forbade the general adoption of such mode of treatment. The effect of the Crossness experiments was to afford a forcible illustration of the proposition, that purification can only be purchased at the sacrifice of solvency. It is true Mr. Rawson, the General Manager of the Native Guano Company asserted in discussion last year upon the reading of Mr. Shelford's Paper at the Institution, that the Company having proved the efficacy of the process as a purifier, would, if they had been given time, have also proved its value as a source of manurial profit. But, even assuming that the Company could produce a paying manure, which is more than doubtful, they could never do so concurrently with the production of a purified effluent. The two results of profit, or even solvency, and purification, are inconsistent and incompatible with each other. First, there is a bankrupt expenditure upon chemicals, and the sewage is purified, then money runs short, chemicals are stinted to save their cost, and the sewage returns to its native impurity. Or, it may be for a time, perhaps, that a sort of intermediate course is adopted, which, while aiming at economy of chemicals, at the same time attempts a partial purification; but this can only be a postponement of the inevitable result, for just as surely as the margin of indebtedness becomes reduced, so the margin of impurity is increased.

This proposition receives an apt illustration in the quasi-defence set up by Mr. Shelford, on behalf of the A B C system, in his Paper already referred to. Starting with the statement that the effluent was extremely good, he attributed the financial failure of the process at Crossness to the extravagant use of chemicals there as compared with Leamington, namely, 31.8 lbs. per 1,000 gallons at the former, as compared with 1.86 lbs. per 1,000 gallons at the latter place. That is to say, that when, as at Crossness, the admixture of chemicals was ruinously extravagant, the quality of an effluent derived from dilute sewage was very good; but when, as at Leamington, the chemicals were sparingly apportioned, the quality of the effluent was notoriously bad, although even under the latter state of things it does not appear that the Company were enabled to show any commercial profit.

The experience also at Hastings, Southampton, Bolton, and Leeds, all tends to point to the same conclusions. Great things were expected, and have been asserted, of the operation of the process at Leeds; but on account of the impossibility of obtaining a sale of the manure, the contract between the Corporation and the Native Guano Company has been annulled. After two or three years intermittent experience of the A B C system, the Leeds Corporation have discovered that the process can purify, but has not paid, and it has therefore been abandoned by mutual consent.

Before finally dismissing this process, the value of the manures produced by it remains to be considered. Some years ago Dr. Voelcker, in the Journal of the Royal Agricultural Society, thus stated the results of his analyses of five samples:

		£	*s.*	*d.*	
No. 1 sample was worth		0	18	6	per ton.
" 2 " "		1	13	6	"
" 3 " "		0	14	0	"
" 4 " "		0	18	6	"
" 5 " "		0	14	6	"

Basing the value on a comparison of the manure with phosphate of lime at £10 a ton, and ammonia at £60 a ton, Mr. Keates, the Chemist of the Metropolitan Board of Works, put the value, as has been already observed, at 20*s.*, so that he and Dr. Voelcker are practically agreed in their valuations. These, then, may be taken to be the theoretical values of the manure per ton. But in speculating upon the probability of profit, it is not the theoretical but the market value which must be considered. This fact was well put by the Chairman of the Council of the Society of Arts in his in-

augural address on the 15th of November, 1876, when alluding to the conference held in May last on the health and sewage of towns. He remarked that exaggerated notions had been dispelled by experience, and the public were beginning to learn that, in the disposal of sewage, profit must not be looked for; and that at Leeds, where perhaps more experience had been gained on an extended scale than at any other town, the difficulty had been to dispose of the solid precipitate, arrangements having recently been made with a contractor to purchase it and take it away at 12*s.* a ton, a sum wholly inadequate to defray the cost of treatment.

Enough has been said to illustrate the limits of the capacity of the A B C process, although the results which have been stated could be fortified by much additional evidence.

3. *Sulphate of Alumina and Milk of Lime* (as worked by the General Sewage Manure Company, Limited).—The following particulars of this process as practised at Coventry, obtained at a personal inspection of the works last year, will sufficiently indicate the limits of its ability to purify sewage and produce a profitable manure. The total quantity of sludge precipitated daily is twenty-two tons, of which eleven tons, or one-half only is dried by Milburn's apparatus, the remaining half being deposited in a semi-solid state upon the adjacent land, or occasionally disposed of in small quantities at 4*s.* a load, or given away to the farmers. The drying grounds were offensive in their character, and the effluent which passed from the tanks into the river Sherburne was insufficiently purified, on account of the sparing admixture of chemicals, the Company being compelled to keep them down in order to avoid the cost contingent on their use. It was stated indeed that the manurial product was in some cases worth as much as £15 a ton, but that, in order to obtain this extreme value, it had to be previously fortified. In other words, the money had first to be put into the sewage, and then taken out again minus the waste.

4. *Phosphate of Alumina* (as practised by the Phosphate Sewage Company, Limited).—This process, the subject of a patent by Messrs. David Forbes and Astley Paston Price, professes to accomplish the defecation and utilization of sewage, either by precipitation and irrigation, or by means of precipitation only. With regard to its ability to produce a satisfactory effluent, the following analysis of the effluent discharged during certain trials at Tottenham, the results being stated in grains per imperial gallon, shows that it does not attain the degree of purity considered necessary by the Rivers Pollution Commissioners:

Organic matter:		
In solution..	5.74	
In suspension.....	none	
		5.74
Mineral matter:		
In solution.....	57.71	
In suspension.....	none	
		57.71
Total organic and mineral matter		63.45
Organic nitrogen:		
In solution.....	0.47	
In suspension.....	none	
Total organic nitrogen.....		0.47
Equal to ammonia....	0.57	
Saline ammonia.....	3.32	
Total nitrogen calculated as ammonia.	3.89	

It is difficult to ascertain to what extent this Company professes to deal with sewage apart from irrigation. After having been the subject of experiment at Tottenham in 1871, when the foregoing analysis was obtained, the phosphate process was subsequently, in 1873, applied to the treatment of a small portion of the metropolitan sewage, previously to its being passed upon the Lodge Farm at Barking. When the works were visited by the shareholders and others, after having been in operation for seven months, favourable opinions with regard to the process appeared in most of the leading metropolitan Journals, which were afterwards collected and published in pamphlet form by the Phosphate Sewage Company. The *Times*, December 18th, 1873, thus interprets the aims of the Company: "What does this Company profess to accomplish? Certainly not to arrest all or even the chief portion of the valuable fertilising constituents held in solution in town sewage; indeed, according to Dr. Voelcker's analysis, the

effluent water after the Company have done with it, is quite as rich in manurial matter as it was before. Hence this process does not claim to arrest and collect in a dry, portable form the wealth of organic matter which our cities are now wasting and the land is crying for; though, like some inventions, it does profess to facilitate the application of the liquid in irrigation farming by removing the suspended solid matters which have been found a terrible and, in some cases, an unwholesome nuisance."

This only claims for the process, that it prepares the sewage so as to render the effluent suitable for subsequent distribution upon land; but the preface to the pamphlet goes much further, and asserts its ability to deal independently with sewage, to produce a purified effluent without need of subsequent treatment. An inconsistency of statement seemingly underlies not only the claims put forward by the advocates of these processes, but even the opinions recorded by the chemical experts, namely, that the effluent is equally fit to be passed directly to the stream or to the land.

For instance, Dr. Voelcker, speaking of the clarified sewage of this process, observes, "It is free from any perceptibly disagreeable odour and may be safely run into a watercourse; and having lost none of its mineral fertilising matters, and become slightly richer in saline ammonia, while it has been freed from suspended matters, which greatly interfere with sewage irrigation, the effluent sewage is more valuable, bulk for bulk, for irrigation purposes than the raw liquid."

But if it be true, from the chemical standpoint, that the most fertilising elements of sewage are at the same time the most putrescible in their character—always excepting the ready-formed ammonia retained in the effluent, which is unimportant as regards pollution—how can sewage consistently be described as equally fit for the land or the stream? These propositions appear to be incompatible, and certainly are not reconcilable with the chemical opinions which will shortly be referred to.

However, to recur to the practical history of the process. It was tried in combination with irrigation at the Lodge Farm, Barking, but had to be given up upon the ground of expense.

With regard to its experience at Hertford, where it superseded the lime process, Captain Flower, Engineer to the Lee Conservancy Board, referring to its introduction there, says in a Paper presented to the Conference at the Society of Arts last year, "Since then I have received fewer complaints, but the same remark which I made just now as to the necessity of filtering the effluent produced by chemical process applies here. It is true that it may be said filtration is carried on at Hertford, inasmuch as small coke filters are used, but I think the area is too limited. Here again the sewage is much diluted by infiltration of subsoil water."

This testimony can hardly be considered favorable, so far as the purifying power of the phosphate process is concerned, and no details of its financial results have come to the knowledge of the Author. Suffice it to say, that when the Corporation of Leeds abandoned the A B C process, the Committee advertised, inviting any one who had a better system to propose to test it at the trial tanks, and that amongst others the Phosphate Sewage Company responded to this request, but the process was not tested, the Company either not foreseeing a successful issue, or being unwilling to bear the cost of a trial.

The report of Professor Tanner, the Chemist of the Phosphate Sewage Company, seems to dispose of any claims put forward as to its ability to deal independently with sewage apart from filtration or application to land. Referring to its purifying power, he says it can effect "the production of an effluent water which may be applied direct to land, and is valuable for irrigation purposes; or the effluent, if passed through filter beds of limited area, can be so completely purified, as to be capable of being discharged into any river or stream without causing a nuisance." So while it is stated that the effect of precipitation *per se* is to produce an effluent fit for irrigation, it is also conceded that, if it is to be passed into a stream, a subsequent filtration is necessary for the purpose of preventing a nuisance.

5. *Goodall's Process* (The Clarifying and Utilization of Sewage Company,

Limited) failed at Leeds. It professed to deal with sewage at a cost of 7*s.* 6*d.* per 1,000,000 gallons, but incurred a cost of £4 6*s.* 6*d.* at the experimental tanks, reduced to £2 13*s.* 4*d.* at the larger works.

6. *Bird's Process* (Bird's Sewage Company), applied at Cheltenham and Stroud, was reported unfavorably of by the Rivers Pollution Commissioners with regard to its application at the latter place.

7. *Dugald Campbell's Patent*, 1872.—This process, as well as Whitthread's process, having been brought before the notice of the Institution in the course of last year by Mr. Shelford, M. Inst. C.E., and spoken of in favorable terms as dealing *per se* with sewage requires to be briefly mentioned. The model works at Battersea were the scene of its practical operation, the agents employed being lime and superphosphate, and the sewage treated at all times of a weak description. First as regards the quality of the effluent. This must have been unsatisfactory, for Mr. Shelford observes that "it was more suitable for irrigation than raw sewage." If so it was plainly unfit to be admitted into running water. The process, even when operating upon sewage at all times weak, failed to effect a proper degree of purification. Secondly, as regards the cost of producing so unsatisfactory an effluent, Mr. Shelford's estimate is as follows :

	£.	*s.*	*d.*
Cost of superphosphate and lime per ton of manure	2	13	4
Cost of manufacture	1	8	10
Total cost per ton	4	2	2
Value per ton	4	7	2
Manurial profit	0	5	0

This shows a profit of 5*s.* a ton; but on some occasions the manure yielded a value of only £3 per ton, when of course this slight profit was converted into a loss. From these figures Mr. Shelford deduced the inference that Dugald Campbell's process may be made to pay its own expenses, leaving the public to bear the expense of labor and drying, or £1 8*s.* per ton of manure produced. In order to appreciate this view, it should be applied upon a working scale. Thus, at Leamington the manurial product was put down at 5 tons a day, which, at £1 8*s.* per ton, represents an expense to the public of £7 a day, or £2,555 a year; or, capitalising at slightly more than 5 per cent., a permanent charge upon a capital of £50,000. And this expense, it must be borne in mind, is for the working of a process which, after treatment of weak sewage, produces an effluent more suitable for irrigation than raw sewage.

8. *Whitthread's Process, patented* 1872, was tested at the Tottenham Sewage Works, the agents being dicalcic and monocalcic phosphate, and a little milk of lime. As regards the quality of the effluent, Mr. Shelford observed that it bore out the favorable opinion expressed by Mr. Hope. But Mr. Hope's opinion, as recorded at a meeting of the Social Science Congress of the same year, scarcely appears to be so unqualified in its praise, for he says "although of course it cannot extract the ammonia in solution, it does remove altogether the other forms of 'organic nitrogen' in solution"; and again, that it "to some extent purifies." It is unnecessary to consider the question of cost, for the Company working the patent failed, and though a report of Colonel Cox has been cited in its favor, it was finally abandoned at Tottenham for Hille's process.

There would be no advantage in multiplying evidence beyond the stage which has now been reached, for the practical experience of any of the many chemical processes—and their name is legion—all tends to point the conclusion, namely, the incompetency of any method of chemical treatment to deal independently with sewage. In this respect, what is true of the processes specifically mentioned, is also true of many others which, on account of their number, cannot be referred to; as, for instance, Holden's, Hille's, Lenk's, Suvern's and Scott's.

The dicta of three eminent authorities will fittingly close this section of the Paper.

Frankland (Rivers Pollution Commissioners, first report, p. 52). "The operations of the chemists have, therefore, been directed chiefly to the soluble constituents of sewage; and have had for their object, either the precipitation in a solid form of the valuable, but offensive,

ingredients so as to convert them into portable manure, or, secondly, the rendering them inoffensive by the action of disinfectants. Although these operations have not been altogether unsuccessful, they have hitherto entirely failed in purifying average sewage to such an extent as to render it admissible into running water. We have formed this opinion both from observation of the polluting effect of such chemically purified sewage upon the streams into which it was admitted, and from the amount of putrescible organic matter revealed by the chemical analysis of the sewage after treatment."

Krepp, on "The sewage question," 1867, p. 36. "These instances are enough to prove that solids extracted from fluid sewage cannot be manufactured into dry manure, so as to pay for the trouble. Apart from this, chemical analyses, most carefully conducted, of the results of various processes of deodorisation prove, that none of them have thus far succeeded in precipitating or retaining fertilising parts held in solution, the only substances precipitated being those which are held in suspense, and which would just as well have been retained by the mere mechanical process of filtering."

Corfield, "The treatment and Utilization of Sewage," 2nd edition, p. 522. "All these precipitation processes do, then, to a certain extent, purify the sewage and prevent the pollution of rivers, chiefly by removing the suspended matters from the sewage; but they all leave a very large amount of putrescible matter in the effluent water, and at least all the ammonia contained in the sewage (sometimes they add to it); the greater part of the phosphoric acid is precipitated by some of them, while they increase the hardness of the river water, a matter of great importance if the stream be a small one.

"The manures that they produce are in every case very inferior, as may be expected from the known value of the sewage constituents that can be precipitated. They have all failed in producing valuable manure, because the valuable constituent of sewage *par excellence* is the ammonia, which, of course, invariable totally escapes in the effluent water, and is lost to the manure: this shows the futility of all attempts to utilise sewage by precipitation alone."

Surely this concurrence of opinion, coupled with the results derived from practice, justifies the following limitations of the province of chemistry with regard to the Sewage Question.

1. That no chemical treatment can of its own independent ability deal permanently with sewage upon a practical scale, for these reasons :

   (*a*) Because it either fails to effect purification, or
   (*b*) Because purification is purchased at such a cost as to render the adoption of the process impracticable, or
   (*c*) Because it is impossible to manipulate the enormous accumulations of sludge necessarily incident to treatment by chemicals.

2. That chemical treatment has therefore no province as an independent means of treating sewage, and its adoption must be ancillary to the adoption of some other process.

3. That the province of chemistry must be limited to the treatment of sewage in combination with subsequent natural or artificial filtration of the effluent.

4. That a chemical process may be employed with advantage in the case of irrigation or downward filtration, either, when contiguity to human habitation, or limited surface area, requires a more efficient screening of the raw sewage, before it is passed upon the land, than can be effected by mere subsidence or mechanical extraction. In these cases, however, as in every other where chemicals are introduced, the accumulation of sludge is an objection inseparable from their use.

The Author thinks these views must be recognized at last, as he cannot suppose that the state of things disclosed in the most modern American sanitary textbook, published in January of last year, by the State Board of Massachusetts, will continue to hold good in England. Referring to the numberless array of chemical processes from time to time paraded before the notice of the public, it says (p. 333), "This list might be extended almost indefinitely, and seems to

be limited only by the bounds of human credulity. In France and Gemany the precipitating processes have been given up as inefficient. In England a new 'successful' patent process is hawked about every few months, to be soon found only an addition to the list of failures; and the public is perfectly bewildered by the maze of conflicting statements and propositions."

It is to be hoped that the example of France and Germany may be soon followed in this country.

II. THE APPLICATION OF SEWAGE TO LAND, INCLUDING (1) IRRIGATION, AND (2) INTERMITTENT DOWNWARD FILTRATION.

Irrigation.—It will readily be acknowledged that, where land for the purpose can be obtained at a reasonable cost, the practice of broad irrigation presents at once the most efficient and the most satisfactory means of dealing with the sewage of towns. But though the merits and defects of broad irrigation are now generally understood, there are two points which have recently excited the greatest difference of opinion:

(1) Whether a profit can be derived from it; and

(2) What proportion population should bear to acreage.

With regard to the first point, there is abundant evidence, but of a conflicting character.

Thus, from the Parliamentary Return of 1873, it appears that out of twenty-five places practising irrigation, only two professed to show a profit. Swindon exhibited a profit of £12; but Warwick, the large profit of £1,310. With a view of ascertaining what exceptional circumstances were in operation to take Warwick out of the general category, the Author visited Warwick in the course of last year, when the following facts were obtained. The farm was worked up to Michaelmas 1875 by the Town Council, and was then abandoned because it had proven to be a dead loss. It has since been taken by a limited company, composed of members of the Town Council, and no statistics can be had as to its subsequent financial results. The population is 11,000; the area of the sewage farm, 133 acres; the rent paid, £3 10*s.* an acre; the sewage is very weak; the land is clay; rough surface irrigation is practised. Vegetables are grown, but there is no sale for them; and to improve the financial position, a dairy farm was just being started. The effluent goes to the Avon.

The Conference of the Society of Arts last May on the health and sewage of towns was supplied with numerous returns as to the various methods of treating sewage. It had the additional advantage of a Paper by Mr. W. Hope, V.C., as to the profit derivable from sewage agriculture, and similar communications referring to the Beddington and Wrexham farms from Dr. Carpenter and Colonel Jones, V.C., Assoc. Inst. C.E. Consequently the judgment pronounced by the Conference upon this point may be taken as based upon a considerable accumulation of the most recent evidence. It is to the following effect: "It is essential, however, to bear in mind that a profit should not be looked for by the locality establishing the sewage farm and only a moderate one by the farmer." The Author adopts this view; but, before leaving the question of the agricultural value of sewage, he would wish to invite serious consideration to the question of the real value of sewage, as compared with water, for the purposes of irrigation. The use of water as an irrigant is well known, and its effect as a fructifying agent upon crops of a certain kind well established. Can it be shown that sewage produces any appreciable effect, beyond that which might be obtained by irrigation with an equal quantity of water? The kind of vegetable growth produced most successfully upon sewage farms is just that which might be expected to thrive upon water irrigation; succulent roots, grasses, the cabbage tribe, and plants absorbing into their system a large amount of water, are those which flourish best, and the effect upon plants requiring a higher form of nourishment is by many considered doubtful. The measure of nutriment in sewage is the nitrogen it contains, and this exists in the ammonia ready-formed by decomposition which has advanced more or less; and in the organic compounds containing nitrogen may appear as ammonia as the result of subsequent decomposition. These are

the pabula of the plant, so far as that particular kind of nourishment is concerned; but is it certain that the nitrogen applied to the land in sewage is actually taken up by the growing plants? Does such nitrogen form indeed the active nourishing principle of the sewage, so far as the vegetation grown upon the sewaged area is concerned? Upon these points it appears that uncertainty exists, even if evidence be not forthcoming to show that the nitrogen does really not produce the effect ascribed to it, and that sewage, therefore, as an irrigant, possesses little or no real value beyond water for agricultural purposes.

In a report upon the results of the use of sewage at the Barking farm, by Mr. Morgan, dated September, 1871, there appear two analyses by Dr. Frankland; one of the sewage employed on the farm, and the other of the effluent water draining away from the farm.

RESULTS OF ANALYSIS EXPRESSED IN PARTS PER 100,000.

Description.	Total Solid Matters in Solution.	Organic Carbon.	Organic Nitrogen.	Ammonia.	Nitrogen as Nitrate and Nitrites.	Total combined Nitrogen.	Chlorine.
Barking.							
Sewage from wooden carriers on E., 10.30 A.M., Sept. 5th, 1871	96.1	2.583	1.752	5.450	..	6.240	22.1
Effluent water from main outfall on F., 1.30 P.M., Sept. 5th, 1871	91.3	0.676	0.198	0.005	4.143	4.345	13.4

Description.	Hardness.			Remarks.
	Temporary.	Permanent.	Total.	
Barking.				
Sewage from wooden carriers on E., 10.30 A.M., Sept. 5th, 1871	..	..	..	Suspended matter: Mineral. 5.24 | Organic. 5.60 | Total. 10.84
Effluent water from main outfall on F., 1.30 P.M., Sept. 5th, 1871	7	39.3	40.0	Few suspended parts.

These analyses present this remarkable fact that, whereas the sewage contains 6.24 of combined nitrogen, the effluent water contains no less than 4.34 of the same ingredient, or more than 70 per cent. of the whole quantity. In considering what amount of the remaining 30 per cent. can be credited to the fructification of the crop, it must be remembered that by far the greater part of this nitrogen was in the form of ammonia—really carbonate of ammonia, a volatile salt—and it can hardly, therefore, be believed that some portion of this was not dispersed into the atmosphere; so that, in reality, much less nitrogen than is shown by these analysis would actually find its way to assimilation with the plant. Whether or not experience bears out this view, it practically raises the questions: Is sewage irrigation anything more than water irrigation; and does sewage, as applied to land, possess any special monetary value? If not, it goes far to explain the financial difficulty which has attached to the working of sewage farms, for it shows that the money paid for the use of sewage as a fertilising agent has been thrown away, and that water, if employed in its stead

for the cultivation of crops, would have proved nearly, if not quite, as efficacious.

The proportion of population to acreage is a question upon which, although it has formed the subject of much discussion, it is impossible to do more than found a very general conclusion. In fact the proposition is clearly governed by such a variety of circumstances, as character of soil, depth of natural drainage, contiguity to population, strength of sewage, &c., that it would be folly to attempt to set up any definite standard. All that can be done therefore, is to indicate what would be a safe proportion under ordinary conditions, by taking the proportions adopted in various cases, and finding a mean. For example, taking the eleven towns referred to in the Report of the Birmingham Sewage Committee, where the relative proportions of population and acreage are stated, and striking an average between them, a mean result is obtained of 103 persons to an acre. Perhaps, bearing in mind the two things required of broad irrigation, namely, sufficient purification and agricultural return, 100 persons to an acre, the soil being favorable, is a safe proportion for general practice. At the same time if the eleven instances referred to, and which have yielded the above standard, be analytically examined, it will appear how impossible it is to lay down a universal rule. At Bury, with a population of 15,000, the proportion runs as high as 577 persons to an acre, and there is absolutely no effluent; while at Banbury, population 11,500, it is 90 to the acre. At Warwick, population 11,000, 81 to the acre, and at Romford, population 7,000, only 58 to the acre. The explanation of the enormous difference in the proportion adopted in various localities depends no doubt to some extent on the porosity or retentiveness of the soil, but much more upon the depth of soil which remains clear above the level of the subsoil water. For even in broad irrigation it is not merely the surface contact of the sewage with the soil, assisted by the oxidising influence of vegetation, which conduces to the resolution of sewage into its innocuous elements, but above all, its passage through that aerated earth filter which intervenes between the surface and the subsoil water. This is the principle upon which Intermittent Downward Filtration claims to proceed, the capacity of which will now be considered.

2. *Intermittent Downward Filtration* is a new term coined to express a very old meaning, and which claims as a novelty a very old principle. There is nothing in Intermittent Downward Filtration, so far as principle is concerned, which did not exist before the so-called invention came forth from the laboratory of the Rivers Pollution Commissioners. Intermittent Downward Filtration is merely the artificial production of what is frequently found in nature, and its only claim to novelty lies in the fact, that though found in some instances, it does not exist in all. There is nothing in Intermittent Downward Filtration which is not sometimes common to irrigation; in fact irrigation sometimes is Intermittent Downward Filtration, in the strongest sense in which the latter system is explained by its advocates. To illustrate this: The theory of those under whose patronage Intermittent Downward Filtration has been introduced as a new system is shortly this, that if deep drainage, say to the depth of six feet, be adopted, the proportion of population to acreage may be enormously increased, and, consequently, the surface area correspondingly diminished. But this is the same old principle which accounts for the varying proportions in the practice of irrigation. Take a professed example of Intermittent Downward Filtration, namely, Merthyr Tydvil, and one of irrigation, namely, the Eton Sewage Farm. Where is the difference which constitutes irrigation in one case, and Downward Filtration in the other, save that the conditions at Eton are those of nature and at Merthyr they are imitations of nature? At Merthyr there is artificial drainage to the depth of six feet, at Eton there is natural drainage to the depth of eight feet, so that, as far as deep drainage goes, irrigation has the best of it, and drainage therefore to a depth of six feet can scarcely be claimed as a startling innovation. At Merthyr Tydvil the filter is divided into plots, and the sewage is applied intermittently to each plot. At Eton the sewage is turned on to one portion of the farm for a while, and then diverted and poured upon another section. The Eton Sewage Farm fulfills all the

conditions of Intermittent Downward Filtration, and yet it is called irrigation. But it is not the name that Intermittent Downward Filtration bears which calls for any serious comment; it is rather the careless and erroneous pretensions asserted with regard to its capacity which really demand serious criticism and investigation. Certain standards have been persistently put forward as indicating the capacity of the new system, never justified in the extent to which they go, either by the experiments out of which the new system was evolved, or by the subsequent experience of its practical operation.

To make the bearing of the argument plain, by which it is proposed to establish the fallacious character given to Intermittent Downward Filtration, it will be advisable to state the points to which attention is sought to be particularly directed. First, it may be premised that when reference is made to the standards set up by the Rivers Pollution Commissioners, it is not to be inferred from the use of the word "standard" that the Commissioners have actually committed themselves to the publication of any absolute rule. What is intended to be conveyed is that they have stated certain authoritative opinions, as to the proportion population should bear to acreage, first of all with certain reservations, but afterwards in such dogmatic terms that the record of such opinions has become little removed in its character from the promulgation of a definite standard. In the first instance the proportion was variously stated at from 2,000 to 3,300 persons to an acre, with certain reservations as to its effects pending the application of the experiments on a practical scale.

It was afterwards claimed that such a test had been afforded by the experience of the system at Merthyr Tydvil, and all reservations were consequently withdrawn. Accordingly, it is proposed to show first, that the proportions stated by the Commissioners in their reports were not justified by the experiments upon which they are based; secondly, to prove that the experience of Merthyr, instead of confirming, entirely refuted the accuracy of the suggested standards; and thirdly, to establish the fact, that in no case does experience warrant the permanent adoption of a larger proportion than 500 or 600 persons to the acre.

It appears from the First Report of the Rivers Pollution Commissioners, that a number of experiments were instituted in their laboratory by Dr. Frankland, extending over the years 1868–1869, for the purpose of determining the effect of the downward filtration of sewage through various soils, of which the following short summary will suffice:—Satisfactory purification of London sewage can be effected through fifteen feet of sand and chalk, at the rate of 5.6 gallons of sewage per cubic yard in twenty-four hours; through Beddington soil, at the rate of 7.6 gallons; through Dursley soil, at the rate of 9.9 gallons; through Hambrook soil, barely at the rate of 4.4 gallons; while through Barking soil, purification was not effected at the rate of 3.8 gallons; and Leyland peat might be educated into effecting purification at the rate of four gallons.

Before examining these results with a view to found a logical conclusion upon them, it is submitted that whatever the mathematical mean may prove to be, it is still too high, looking to the fact that it is derived from laboratory experiments. A certain margin must be allowed for the well-known unreliability of such experiments, as affording an absolute criterion of what will occur in practice. Such an element of unreliability must be a necessary incident of all laboratory experiments, for the experiment is freed from those disturbing influences which attach to a similar operation on a large scale, exposed to the ordinary conditions of nature. The chemist who conducts the experiment adjusts everything with a nicety of methodical arrangement which must conduce to the most favorable results. The earth is carefully packed layer upon layer in the glass cylinder, and that irregularity of composition is avoided which almost invariably occurs in the soil forming the natural filter bed. The chemicals are weighed out in scales, and allowed to mingle with the sewage in accurate proportion, and thus that rough-and-ready admixture which distinguishes the practical application is entirely excluded from the experiment. The sewage itself is probably collected by means of a bucket from the top waters of a sewer, and so those heavier particles

are avoided, which, gravitating downwards, pass along the invert, and which distributed upon the land tend to clog the pores of the natural filter. Then again, there is no rainfall to impede filtration, no sunshine to develop noxious exhalations, and no wind to waft such odors in the direction of contiguous habitations. Freed from every disturbing influence, the experiment, proceeding in the uninterrupted quiet of the laboratory, necessarily yields the most favorable results. Laboratory experiments must therefore be discounted before they can be accepted as affording a standard of any real practical value.

But have the experiments referred to been so discounted? On the contrary, they have been strained, and the opinions based upon them exaggerated. To test this: It is clear that for the purpose of arriving at the true indication of the experiments, a process of selection must not be adopted. If a proportion is to be asserted or a standard set up, it must not proceed upon the best or the worst, but upon the mean results afforded by the experiments from which they are derived. But if the six experiments in the Commissioner's Report be grouped together for the purpose of striking an average, the mean result will be too favorable, as two out of the six experiments show negative results. Thus in the case of filtration through Barking soil, not only was no purification effected at the rate of 3.8 gallons per cubic yard in twenty-four hours, but deterioration was actually taking place; so that, for the purpose of obtaining a true mean, a lower figure than 3.8 gallons should be taken to represent the positive result of the experiment. And again, in the case of filtration through Leyland peat, the Commissioners merely express a hope that it might be educated into purifying at the rate of four gallons per cubic yard in twenty-four hours; so that, for the purposes of a true mean, a lower figure than four gallons should be adopted. Nevertheless, both these negative results are for the present purpose treated as though they were positive. Striking an average, the mean obtained indicates that a satisfactory purification of London sewage may be effected at the rate of 5.9 gallons of sewage per cubic yard of soil in twenty-four hours. Now the first suggestion of a standard is to this effect (p. 69 of the same Report): "These experiments upon the filtration of sewage through various materials leave no doubt that this liquid can be effectually purified by such processes. . . . . In this way the sewage of a water-closet town of 10,000 inhabitants, could at a very moderate estimate, be cleansed upon five acres of land if the latter were well drained to the depth of six feet." Setting on one side the discount due to a laboratory experiment, it is clear that, even then, this proportion of population to acreage is not justified in its extent by the mean of the experiments. A population of 10,000 persons to five acres is in the proportion of 2,000 persons to 1 acre. The sewage of these 2,000 persons must be taken at thirty gallons a head, the London water supply, for it was upon London sewage that the experiments were made; and as the water supply represents the amount of dilution, and, consequently, determines the strength of dry-weather sewage, it is important that, in applying the experiments, the same ratio of thirty gallons per head should be preserved. To say, therefore, that the sewage of 2,000 persons can be disposed of every twenty-four hours, upon an acre drained six feet deep, is equivalent to saying that 60,000 gallons of sewage can be purified every twenty-four hours upon 9,680 cubic yards of soil, or that purification can be effected at the rate of 6.2 gallons per cubic yard in twenty-four hours. But the mean of the experiments was only 5.9 gallons, so that the first standard suggested by the Commissioners is in excess of the mean.

The element of exaggeration becomes more manifest in subsequent utterances. For instance, in their First Report, a population of 3,300 is stated to represent the purifying capacity of an acre drained 6 feet deep, which at once raises the proportion to 10 gallons per cubic yard in twenty-four hours, as compared with 5.9 gallons, the mean of the experiments. In their third Report, the Commissioners appear to have cast off the reserve with which they had expressed their conclusions in the first Report, and at the same time to have strained the experiments to their fullest extent. They say: "And 100 acres or more might be needed to

cleanse, certainly to profitably utilise, the drainage of a town of 10,000 people by means of irrigation, it would need but 3 acres of a porous medium 6 feet deep, worked as an intermittent filter, to oxidise and therefore purify the drainage water of such a town, provided the mass of earth through which it percolated were frequently and effectually aerated, and the foul liquid were so added that every part of this aerated filter had its equal share and equal interval of aeration. The laboratory experiments, on which we build our confidence, in filtration thus conducted for cleansing sewage, may be considered conclusive as to the satisfactory and permanent efficacy of the remedy thus provided; and although it is by no means certain that such an apparatus, on the scale needed for a large town, would not itself be a formidable nuisance, yet there is every reason to believe that the water running from it would be sweet and clean."

This extract contains an absolute expression of opinion on the part of the Commissioners, that though the filter might be a nuisance, the sewage of 3,333 persons may be permanently purified upon an acre of land drained 6 feet deep, or that permanent purification can be effected at the rate of 10 gallons per cubic yard in twenty-four hours. But the highest result afforded by the experiments was in the case of the Dursley soil, where purification was effected at the rate of 9.9 gallons per cubic yard in twenty-four hours, while the mean result, as already stated, only justified application at the rate of 5.9 gallons in the same period; and yet the Commissioners have laid down a standard for adoption in practice, which is actually in excess of the highest and most favorable experimental return, and nearly twice as high as the mean result of their experiments. How, then, can it be contended that the experiments have not been strained, and that the conclusions of the Commissioners, even as they stand upon their own experiments, are not fallacious and calculated to mislead?

THE RUSTIC THEORY OF INDUSTRIAL ART.

From "The Architect."

Amongst the art-preachers of the moment who assume the function of revolutionising our industrial design and establishing upon its wreck some better mode, Mr. C. L. Eastlake deserves honourable mention. Indeed he is fully entitled to be called a representative man; for not only is he possessed of a pleasant style of writing—which in itself is half the battle—but he has long indentified himself with the practice of what he preaches, and has met with no inconsiderable share of that success which is the reward of merit.

Such teachers have, as a matter of necessity, the majority against them; but so much the better. Indeed it is this that contributes more than anything else to the delight of their position, which is of course the delight of battle. This it is that holds them together and stimulates them to exertion. Let them but win the conflict—which happily they are never destined to do—their bond of union becomes dissolved, and internal differences spring up to shift altogether the ground of controversy. Still they may be right, and no doubt in most cases are, in one degree or another, seen to be as clearly right in this perverse world as they could expect to be. In the present case, in particular, we need not hesitate for a moment to assume our reformers to be so far right that the necessity for reform is palpable.

The passive majority on the other side must generally be excused, however, if they fail to understand what the new school would have them to do. The wisdom is not always so considerately developed as might be, and the wit especially—of which there is a great deal too much—is scarcely conductive to patient reflection. It comes to be, therefore, quite a godsend in such cases when some professor of the new faith consents to lay on the shelf for a moment both philosophy and ridicule and to give an in-

stance in plain language of what it is he founds his faith upon. "To this day," thus says Mr. Eastlake in his Spitalfields lecture reported last month, "I know no specimen of native manufacture more satisfactory in construction and more picturesque in appearance than a *rustic cart*. It is always solidly made of stout timber and well hammered iron. . . It serves its object well and honestly and has filled the page of many an artist's sketch book. . . That is the kind of work we should try and revive in this country." Now no one can resonably plead misunderstanding here. The idea which is expressed is perfectly plain to the critical mind; it is at once seen to possess, now that we have it fairly before us, the true ring of the reforming doctrine of the class which Mr. Eastlake represents; and it constitutes a specific illustration of that doctrine which will bear handling. Moreover it is an illustration which one may fairly be called upon, in spite of temptation, to handle considerately —with as much of sober wisdom and as little hysterical wit as may be; it being, as we have already ventured to hint, much better just now, in dealing with whatever proposals of revolutionary art, to rely upon the solid sense of argument than to trust to the empty sound of pleasantry.

The rustic cart doctrine, then, is simply this. The industrial fine-art proper to the nineteenth century, as practiced in England, has been a feeble and vulgar sham, expending its small stock of taste —a very small stock indeed—upon hollow affectation and mistaken because ignorant copyism; from which deplorable and indeed contemptible state of things there is but one way of escape, namely, a return to such almost primitive simplicity as shall admit of our being at least honest if we cannot be refined. Honesty, in other words, is, in industrial art as in all else, the best policy; and consequently dishonesty the worst; let us, therefore, discard all our unrealities of design at whatever cost, and fall back upon what is real however humble.

Let us pitch the Lord Mayor's new state carriage, for instance, into the river, and send him to Court in a rustic cart; an extreme measure, of course, but one which would at any rate do honor to those first principles which a perverted generation has so effectually lost.

It is only an incident in this train of thought that "our forefathers" are taken to have done altogether as well in respect of common art as we do altogether ill. This allegation satisfies a well-known prejudice belonging to our day, without really affecting the question at issue; and it may, therefore, be passed over as a sort of rhetorical flourish. At the same time it may be well to understand what is the presise form in which such ancestor-worship is made to do duty in the case in hand. In old times, then, as it is said, our mechanics, remaining stationary in their native towns and villages, "content to work as their fathers and grandfathers did before them," acquired local habits of workmanship, equivalent in fact to traditionary systems, of which "the result was most beneficial to every branch of industrial art." But "railroads were laid down; schools were set up; cheap newspapers were circulated." The old traditions have thus been broken up, or if any have survived it is "probably because no shams could be practised in them without at once rendering the objects produced useless for their purpose;" and it is to emphasise this statement of the case that Mr. Eastlake introduces his illustration of the rustic cart, after which he goes on very pointedly to say—"Pray don't suppose that I am putting an extreme case; . . . all I desire to point out is that (our) object should be to start from the principles which guided our forefathers in design and construction."

Now, as we have said, it would be idle to ridicule this rustic cart, however easy; and we may also again add that it is unnecessary to mix up the historical question with the artistic. The rustic cart stands for a whole order of unpretentious design-work; and the proposition of our refomers is that we ought to revert to the principles involved in this order of unpretentious design-work, and to throw aside those principles which otherwise are our own. For instance, let cast iron ornament give place to wrought; let the joiner strike his mouldings in the solid instead of planting them on the surface; abandon cement work in imitation of stone; abandon the art of graining and marbling; let "crockery" be

painted by hand ; the contrary of all this is "cheap and nasty work, and a neglect of those honest trades which were once the pride of the British workman."

Mr. Eastlake, in the sequel of his lecture, may no doubt be said to ride off from the simplicity of his doctrine and to dwell rather upon the very different subject of the recent imitative revival of old work—that is, work about a hundred and fifty or two hundred years old—in furniture; but this need not in any way diminish our interest in the original and more important question, how we are to improve the condition of English industrial art by rupudiating our system of counterfeit, and seeking in the crude integrity of the rustic cart a new inspiration. This is a most important and most interesting question, and one that obviously has nothing to do with the graces of Chippendale furniture and Japanese ornaments.

It is not improbable that if Mr. Eastlake had happened to know more of the rustic cart as regards its uses in the farmer's work, and less of it as regards its uses in the painter's picture, he would have hesitated to rest his argument upon this particular illustration. Either the rustic cart is a model of scientific construction and artistic design combined, or it is nothing at all to the purpose. Now the rustic cart of the present day is, more than most things, a subject of diversity of design, variety of quality, and keen competition in price. • In a word, if Mr. Eastlake thinks it worth his while to go to some agricultural show and cotemplate the article in its utilitarian integrity, he will probably decline to pin his faith to it any longer. The more purely scientific it is in construction, certainly the less he would like it; and the more completely it is devoid of even the humblest grace, the less would it suit the artist's sketch-book. If, however, it is merely the *old-fashioned* or rather the *old* cart that he would wish to be understood to refer to, then the question arises whether it is not in reality the old-fashioned and used up character of picturesqueness, the oddity and quaintness and obsoleteness, that he admires, and not by any means the mere sturdy substantiality and honesty of crude workmanship. Then, if it is truly this element of picturesqueness that affords him pleasure —that which, beyond doubt, causes it to have "filled the page of many an artist's sketch-book"—the further question arises whether he ought not to prefer the cart of romote localities—of Wales or Ireland, of Normandy or Sweden, of Spain or Siberian Tartary. As for honesty of workmanship, all these are probably very much alike, but in the quality of quaintness they differ to any extent you wish. Not only so, but if he will accept with the cart the carter and the horse, and take his stand upon the indubitable fact that the artist's sketch-book will cheerfully admit Old Dobbin and Hodge —and all the more cheerfully the more obsolete and forlorn they look—while it will emphatically refuse to admit on any terms my lord and his glossy bay —we may surely ask whether Mr. Eastlake is prepared, like an artistic Marat, to advocate the abolition of my lord and his glossy bay as simulacra and impostures, and to throw civilization back upon Hodge and melancholy Dobbin as the honest realities which have descended to us from our forefathers.

Upon the desirableness of our escaping from a condition of artistic pretence and vulgarity there cannot be two opinions; but whether we are to escape from it by adopting the style of the rustic cart is quite another question. It must not be forgotten that the acceptance of anything whatever as a model for imitation is still an act of pretence and counterfeit; and what the reformers of industrial art have to accomplish is the very difficult task of separating the inner principle from the outer example—a task so difficult that it has certainly never been accomplished since the memorable time when the Revival of Arts and Letters endowed the modern world with the artistic wealth of the antique. "When we have mastered those principles (which guided our forefathers)," says Mr. Eastlake, "and learned to work well and thoroughly in *any one style of a by-gone age* it will be time enough to think of inventing a new style." Instead of *principles* it is manifestedly *practice* that is here meant.

If our reformers can do no more than direct us to the adroit imitation of one period of past work instead of another, so far so good, but surely no farther— we may escape, to a certain extent, from vulgarities, but not from shams.

ON FRICTION BETWEEN SURFACES AT LOW SPEEDS.

By FLEEMING JENKIN, F.R.S., and J. A. EWING.

Proceedings of the Royal Society.

The common belief regarding friction, which is based on the researches of Coulomb and Morin, is that between surfaces in motion the friction is independent of the velocity, but that the force required to start the sliding is (in some cases at least) greater than the force required to overcome friction during motion; in other words, the static coefficient is usually considered to be greater than the kinetic. It occurred to the authors that there might possibly be continuity between the two kinds of friction, instead of an abrupt change at the instant in which motion begins. We should thus expect that when the relative motion of the surfaces is very slow there will be a gradual increase of friction as the velocity diminishes. Whether any such increase takes place at very low speeds is left an open question by the experiments of Coulomb and Morin, whose methods did not enable definite measurements of the friction to be made when the velocity was exceedingly small. The authors have succeeded in measuring the friction between surfaces moving with as low a velocity as one five-thousandth of a foot per second, and have found that in certain cases there is decided increase in the coefficient of friction as the velocity diminishes.

The apparatus made use of consisted of a cast-iron disk 2 feet in diameter and weighing 86.2 lbs., supported on a steel axle whose ends were less than one tenth of an inch in diameter. These ends were supported in bearings which consisted of rectangular notches cut in pieces of the material whose friction against steel was to be measured. The disk was caused to revolve and then left to itself, when it came to rest in consequence of the friction on the ends of the axle. The rate of retardation was found as follows:—A strip of paper 2½ inches broad was stretched round the periphery of the disk, and a pendulum was caused to swing across this paper in a plane perpendicular to that of the disk. On the pendulum was fastened a fine glass siphon, one end of which dipped into a box containing ink, whilst the other stood at a short distance from the paper strip, across which it was carried as the pendulum oscillated. By keeping the ink-box strongly electrified ink was deposited on the paper by the point of the siphon in a rapid succession of fine spots. By this means, without the introduction of any new source of friction, a permanent record was made of the resultant motion of the pendulum and the revolving disk. This frictionless method of recording was designed by Sir William Thomson for telegraphic purposes, and is employed in his siphon recorder. From the curve drawn in this way it was easy to determine the rate of retardation of the disk (and therefore the friction) corresponding to various velocities of the rubbing surfaces. The lowest velocity for which the determinations were definite was about 0.0002 foot per second, and the highest velocity to which the experiments extended was 0.01 foot per second. The surfaces examined were steel on steel, steel on brass, steel on agate, steel on beech, and steel on greenheart—in each case under the three conditions, dry, oiled, and wet with water. In the cases steel on beech oiled or wet with water, and steel on greenheart oiled or wet with water, the coefficient of friction increased as the velocity diminished between the two limits given above, the increase amounting to about twenty per cent. of the lower value. It appeared that at the higher limit of velocity there was little further tendency to change in the coefficient; but it is impossible to say how much additional change might take place between the lower limit of the velocity and rest. In the case of steel on agate wet with water there was a similar but much less marked increase of friction as the velocity decreased; and in the case of steel on steel oiled there was a slight and somewhat uncertain change of the opposite character—that is, a decrease of friction as the velocity decreased. This case, however, would require further ex-

amination. In all other cases the friction seemed to be perfectly constant and independent of the velocity. Out of all the sets of circumstances investigated, the only ones in which there was a large difference between the static and kinetic values of the coefficient of friction were those in which a decided increase was observed in the kinetic value as the speed decreased. This result renders it exceedingly probable that there is continuity between the two kinds of friction.

TORPEDO LAUNCHES.

From "Engineering."

THE very interesting paper which Mr. Donaldson, of the well-known firm of Thornycroft and Co., of Chiswick, read before the United Service Institution, has called public attention forcibly to the subject of torpedo warfare. As a builder of launches, not torpedoes, Mr. Donaldson speaks only of that which he knows, so he broaches no new theory of warfare, but contents himself with describing things already done or built.

A more valuable contribution to our knowledge could hardly be made than his modest list of torpedo launches built and ordered, and we shall take the earliest opportunity of printing it *in extenso*, together with the diagrams by which the lecture was illustrated.

The famous river launch Miranda, built in 1871, and which in the following year was found to go at 18.65 miles, or nearly 16¼ knots, per hour, though less than 50 ft. in length, was apparently the prototype of the modern fast torpedo launch. Built of thin steel plates and fitted as lightly as possible, engined to the utmost, and conspicuous in every detail for that perfection in design and workmanship which is never absent from the work of those who achieve, in new fields, such rapid and complete success as Messrs. Thornycroft have attained, the Miranda was seen at once to offer just what torpedoists required. The Norwegian Government, in 1873, gave the first order for a boat of 57 ft. length, 7 ft. 6 in. beam, and 3 ft. draught and having —as appeared in the final trials—the remarkable speed of 17¼ miles, or 14.9 knots, with an indicated horse power of about 90. The engine-room and what might be called the steering compartment, were protected against musketry fire by $\frac{3}{16}$ in. steel plaiting, and safety was further sought in division into six water-tight compartments. This boat was armed with a towing torpedo attached to the funnel, in such a manner as to diverge at an angle from the boat's course, in the same manner as the Harvey torpedo. Like some other apparently unpromising types, the Norwegian launch have very respectable sea-going qualities. Two similar boats were shortly after built for the Swedish and Danish Governments, that for the Danish Government, and probably the other also, being armed in a manner resembling the boat first described.

The next boats built were larger and more powerful, the class comprising, apparently, two for France and one for Austria. The length was 67 ft.; beam, 8 ft. 6 in.; draught, 4 ft. 3 in.; indicated horse power about 200; and speed over 18 knots. The armament consisted of spar torpedoes, fired by electricity, either automatically upon contact, or at pleasure of the operator. The French boats on their passage out crossed boldly from the Thames to Cherbourg, without hugging the coast, and in February and March of this year they proved their efficiency in some experiments which have attracted a great deal of attention. The Bayonnaise, an old wooden frigate, "which had been damaged in one of the earlier experiments, and was on this occasion kept afloat by means of empty casks," was towed by another vessel at a speed of about six knots, and was attacked by one of the torpedo boats. As the rehearsal was incomplete, so far as any attempts at defence were concerned, a hole was made in the Bayonnaise large enough "to admit a full sized omnibus" —a result not at all surprising, nor calculated to teach anything not known

many years back, but which, from its sensational character, drew great attention from the English press. (The accounts published at the time were absurdly exaggerated, as any one reading them might see.) The real interest of the experiment, and the point it was intended to throw light upon was the effect of the explosion upon the attacking boat. "At the moment of explosion a slight shock was felt, and immediately afterwards a large wave, was upheaved between the Bayonnaise and the torpedo boat, *which was driven backward a considerable distance* and completely covered with water, so much so that M. Lemoine and his brave companions for the moment could not say whether they had gone to the bottom or not. This state of doubt was soon dispelled, however, and M. Lemoine steamed slowly off to report himself on board the Coligny." The torpedo used contained 15 kilogrammes of damp gun-cotton, and was fired 8½ ft. below the surface, at the end of a steel pole 40 ft. long. Six other boats, considerably larger—indeed the largest torpedo launches yet commenced—are now in hand for the French Government, to be armed, it is believed, in the same way. They are 87 ft. long by 10 ft. 6 in. beam, and are more heavily built than any yet constructed, and Messrs. Thornycroft guarantee they shall maintain the speed of 18 knots during a three hours run. The plates and frames below water are galvanized.

Between the earlier and later French boats come some for the Dutch and Italian Governments; they measure 76 ft. by 10 ft.; and are to have a guaranteed speed of 18 knots. The Dutch boats will be armed with spar or outrigger torpedoes, like the French, and the Italian with the Whitehead or fish torpedo.

Nearly last on the list comes the single small torpedo craft yet constructed for our Government, the Lightning, whose successful speed trials on the Thames attracted attention a few weeks back, when (running a little light) she attained the speed of 19.4 knots. Her length is 84 ft., and breadth 10 ft. 10 in., with a draught of 5 ft., and indicated horse power of about 350. As might be expected from her size she has considerable sea-going qualities and internal accomodations, but she can hardly be called a steam launch. She will be armed with the Whitehead torpedo to be discharged *from the deck* forward.

It cannot be doubted that this principle of attack is destined to play a very large part in naval warfare, but those upon whom it now bursts as a new revelation (which it is not, for Messrs. Thornycroft's work, valuable as it is, has been only to render more practicable the application by a principle long since recognized) appear likely to even exaggerate its importance. So far as we can see at present, neither attacks by fast launches nor by Whitehead's torpedoes, discharged under water from other large vessels, are likely to render heavy fighting ships obsolete. They introduce an extremely formidable element of offensive warfare, which no country can afford to neglect, and which England ought to adopt upon an extensive scale, and they set us also a problem of defence of a very difficult, but, we think, not hopeless kind. There has been little time yet for devising means of protection, and it must not be inferred, because none of a perfectly satisfactory kind present themselves at once, that none can be found. Mr. Donaldson, with a natural partiality for fast torpedo launches, advocates their employment for defence as well as for attack; a small squadron of them is to watch around the ironclad, and to stop any of their own kind which may approach. Mr. Donaldson wisely adds that these should be small enough to be hoisted on board the ironclad, but thinks the original 57 ft. type which weighs 7 tons, is not too large for the purpose. Writing some weeks back, we expressed a strong opinion in favour of furnishing ironclads with small torpedo launches of this kind, so we are to a great extent of Mr. Donaldson's opinion, but we believe that other modes of defence may be found as effectual.

The proper use of defensive torpedoes we long since pointed out as a method of meeting this new and terrible danger which might very probably prove successful, and we are not sure that it would not be practicable to surround a ship with such dangers as to make successful approach almost impossible. So long as she is under way a sufficient number of Harvey or other towed torpedoes might well make her almost unapproachable from behind, and for the greater part of her length upon

either beam, and similar protection round the bow is not inconceivable. As soon as the problem is clearly apprehended—that is, how to make it impossible for a launch to approach without itself running foul of a torpedo before its own can take effect—we suspect it will not be long before a solution is found. The Whitehead torpedo itself might perhaps be utilised in this way, by treating it as a divergent towing torpedo until the launch approached, when directed and set in motion by electric communication through the towing line, it might be sent on ahead of the ship, or caused to assume any position likely to produce collision with the launch. It must be remembered that eluding the torpedoes themselves would not necessarily save the launch; if properly arranged she could hardly fail to foul one or other of their tow-ropes, and thus produce a collision with a torpedo. Not with any idea of suggesting an actual plan, but merely to show how readily such modes of defence suggest themselves, it may be worth while to ask whether two strings of torpedoes, towing one behind the other, might not be kept advancing in parallel lines with the ship, the leading one of each line being towed at a high angle of divergence from, say, the edge of a spar projecting beyond the ship's stem. It might not be too fanciful to conceive the line prolonged forwards on each side by self-moving torpedoes, controlled electrically from the ship. It is not easy to see how a launch could encounter such a defence without very grave danger. Whether any modification of this idea could give partial security against the submarine attack of the Whitehead torpedo might also be worth considering.

Another means of defence of more familiar kind is open to the attacked ironclad, viz., artillery fire. Of the value of this, as against a swift-moving launch, many authorities, including Mr. Donaldson, entertain a poor opinion. But amongst skilled gunnery officers this view is far from being universal, and the strongest advocate of torpedoes will admit that if the guns can only be fired often enough, while the launch is within range, there must be a good chance of destroying her. That the Devastation, with nothing but four 35-ton guns, would have little chance of hitting her pigmy foe, may be conceded, but all ironclads are not armed in that way, and there is no reason why any should be. Henceforth, every ship should carry as large a number as possible of torpedo guns upon the upper deck, expressly designed for accuracy and rapidity of handling. Since they would be required to fire projectiles of a small size, Gatling guns modified might answer the purpose. The object will be to fire as many shots as possible during the space of about three minutes, and it is obvious that, with proper appliances, a very large number might be got off in that time—of which one at least might find its mark.

Whether still more radical measures can be taken tending to make the ship itself torpedo-proof, is a question we are not now disposed to discuss, and which, indeed, we have small hope of seeing answered satisfactorily. Enough has been said to show that the navies of the world are not necessarily at the mercy of Messrs. Thornycroft's wonderful little vessels, while as to attacks by the fish torpedo, if the defence is as yet more difficult, the attack is more uncertain.

Of the launches themselves we can only repeat our high opinion of their value, and our regret that England is so far behind in this branch of preparation. As Mr. Donaldson says, "we have only one fast steam torpedo launch in Her Majesty's Navy, while other nations are providing them by the dozens for the defence of their principal ports."

The Lightning, which we presume costs about 6000*l.*, is an expensive implement to employ upon service of the forlorn hope kind, but it is clearly desirable to have some boats capable of keeping at sea for a few days. The distance at which they are to operate from their base must mainly determine the size, but we look upon all too large to be carried on board ship as valuable only for coast defence. For use with fleets we need something either smaller or larger than the Lightning, and, of course, we prefer the smaller type. As we remarked in a recent number of *Engineering*, the Italians are showing great foresight in naval matters, having the apparent intention of making the Mediterranean an Italian lake, and a very fair idea of how to set about it. In this matter of carried torpedo launches

as in the construction of iron-clads of the very heaviest class, we have allowed them to get the lead, and the two Italian inflexibles, the Duilio and Dandolo, are each, we believe, to be fitted with two of the launches described by Mr. Donaldson. They are too large for hoisting up in the usual way, but are to be carried, as is well known, in a sort of dock in the after part of the ship—an arrangement of a very novel kind, perhaps open to criticism.

Messrs. Thornycroft's achievements need inspire no panic, but they require a more earnest attention than they appear to have received yet from the Admiralty—if English supremacy on the sea is to be maintained.

MOMENTUM AND VIS VIVA.

By J. J. SKINNER, C.E., Ph.D., Instructor of Mathematics in Sheffield Scientific School, New Haven, Conn.

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I.

To the mind of a student anxious to comprehend as thoroughly as may be the subjects of his inquiry, the treatment of *momentum* and *vis viva* by the books on physics and mechanics is extremely unsatisfactory. He rises from his study feeling that there is something left unexplained; and failing to find anywhere an adequate answer to his questions, he is fain to conclude that there is some mystery in the case which will never be quite solved. Several text-books, held in high esteem in our schools and colleges, say that the *momentum* of a moving body is its *quantity of motion*, and that it is equal to the mass of the body multiplied by its velocity. For instance, if a body equal to 10 units of mass have a velocity of 10 feet per second, its *momentum* is said to be equal to $10 \times 10 = 100$. But 100 what? Is it 100 feet, or 100 pounds, or 100 of something else? If *momentum* is a *quantity of motion* it certainly cannot be 100 pounds, and it is quite as difficult to say that this body has a *quantity of motion* equal to 100 feet. And here the majority of the books leave us without giving the first idea as to what the unit of the *momentum* is.

In the case of the *vis viva* of a moving body, represented by one-half the mass multiplied by the square of the velocity, an attempt is made to show what its unit is, but nearly all the books fail to point out the essential difference between this unit and the unit of momentum. Thus the result is a confusion of ideas, leading to vagueness and error. It is proposed in this article to attempt an explanation, as nearly complete as possible, of the exact meaning and nature of *momentum* and *vis viva*, as deduced from a few simple and fundamental mechanical principles.

Physicists all agree in calling the product obtained by multiplying the mass of a moving body by its velocity the *momentum* of the body; denoted algebraically by MV. But there is not the same agreement as to what that product really *means*. In order to understand what it does represent let us consider its relation to the force required to impart the velocity V to the mass M originally at rest.

It is a fact familiar to every one, that a body cannot be moved from rest without the application of force, and also that a body already in motion cannot be stopped without the exertion of force. This property of bodies, by virtue of which they resist any change of state, either of rest or motion, is called *inertia*. The common unit of force, with which the intensities of all other mechanical forces, whether producing motion or not, may be compared and measured, is the force capable of producing a pressure of one pound avoirdupois. This unit will be called briefly a pressure of one pound. Now it is clear that if a force equal to one pound pressure act for one second on a certain unit of mass, originally at rest, and impart to the mass a certain unit of velocity, say one foot per

second, then a force equal to two pounds pressure will be required to impart the same velocity in the same time to twice the mass; or three pounds pressure will be required to impart the same velocity in the same time to three times the mass; and so on, the number of pounds pressure being equal to the number of units of mass to which in one second a velocity of one foot per second is imparted. Also, since any force acting on a body produces its own proper effect on the motion of the body, whether any other force act on the body at the same time or not, it is clear that in order to impart to the unit of mass in one second a velocity of two feet per second, by a constant pressure, the intensity of that pressure must be two pounds; or if the velocity given to the unit of mass in one second be three feet per second, the pressure will be three pounds; and so on, the number of pounds pressure being equal to the number of units of velocity given to the units of mass in one second. If, then, a certain pressure acting for one second on say five units of mass give it a velocity of one foot per second, the intensity of that pressure is five pounds; and the intensity of the pressure which could give to the same five units of mass in the same time a velocity say of six feet per second would be six times five pounds, or thirty pounds. And in general, the number of pounds in a constant pressure which can impart in one second to M units of mass a velocity of V feet per second, is equal to the number of units in the product MV.

These laws may be verified by means of Atwood's machine. But it is to be noticed that the pressure, by these laws, is supposed to be all applied in producing motion of the body through space, none of it being employed in producing compression or extension of the mass; since in the case of gravity, by which the laws are verified, the force is uniformly applied to all the particles of the body, and has no tendency to alter its shape.

If, then, any body having M units of mass, be moving with a velocity of V feet per second, no matter how long it may have been moving, or how intense the force which gave it motion, we know that the same velocity *might* have been given to it in one second by a constant pressure of MV pounds, uniformly distributed through its mass. It may further be proved by Atwood's machine that if a body move from rest by the action of a constant pressure for a certain time, and acquire any velocity, and the impelling pressure then cease to act, the body may be brought to rest again in an equal time by an equal constant pressure in the opposite direction. Therefore, in the case of any mass M moving with the velocity V, since the same velocity could have been imparted in one second by a constant pressure of MV pounds, that velocity may be destroyed and the body brought to rest in just one second, by an equal constant pressure of MV pounds, uniformly applied to its mass, in the direction opposite to its motion.

Let us now consider the value of M. By the standard adopted above, the unit of mass is the amount of matter which, acted on for one second by a pressure of one pound, will acquire a velocity of one foot per second. But this amount of matter is not the same as the amount which weighs a pound. In case of a pound weight falling freely in a vacuum we have a pressure of one pound exerted on the amount of matter which weighs a pound, and the velocity at the end of one second will vary slightly with the latitude of the place, and is usually represented in physical formulæ by the small letter g. But if a pressure of one pound acting for one second on the amount of matter which weighs a pound can give it a velocity of g feet per second, it follows from principles already explained that the same pressure of one pound can produce, in one second, a velocity of one foot per second in the amount of matter weighing g pounds. Hence g pounds of matter is our unit of mass. The number of units of mass, then, in any number of pounds of matter is equal to the number of pounds divided by g. Or if G be the number of pounds in the weight of a body, and M the number of units of mass, we have

$$M=\frac{G}{g}.$$

If, then, in the expression MV we substitute this value of M, we have $MV=\frac{GV}{g}$; and the number of units in this expression represents the number of pounds constant pressure, uniformly dis-

tributed, by which the velocity V *might* have been imparted in just one second of time to the mass M, weighing G pounds, or by which the mass M, weighing G pounds *might* be brought to rest in the same time from the velocity V, if an equal constant pressure were similarly applied in the direction opposite to its motion. That is exactly what the product $MV=\frac{GV}{g}$ which is the *momentum* of the body, represents. And the *momentum* of the body has properly no other meaning.

The value of g, found by experiment, is approximately $32\frac{1}{6}$ feet. Substituting this value we have $MV=\frac{GV}{32\frac{1}{6}}$ If we say then that a body which contains ten units of mass, (and whose weight would therefore be $10\times32\frac{1}{6}$ pounds $=321\frac{2}{3}$ pounds), and whose velocity is equal to ten feet per second, has a momentum equal to $10\times10=100$, we mean that the same velocity *might* have been imparted to the same mass in just one second by a uniformly distributed constant pressure equal to 100 pounds; or that if the body were brought to rest in just one second by a constant pressure similarly applied, but exerted in a direction opposite to its motion, the intensity of that pressure would be equal to 100 pounds. And we do not mean anything else whatsoever.

Let us now consider the *vis viva* of a moving body. It may be shown by means of Atwood's machine that in order to overcome, by a constant pressure, the inertia of a given mass of matter at rest, and cause it to move with a given velocity, a certain definite number of *units of work* must be performed on it. And this is true whether the velocity be imparted in one second or any number of seconds, the number of *units of work* which must be performed to produce in the mass the given velocity is the same in all cases. By a *unit of work* is meant the exertion of a unit of pressure through a unit of distance, usually taken as a pressure of one pound through a distance of one foot; and the number of units of work, called usually foot-pounds, performed by any force, will be equal to the number of pounds pressure produced, multiplied by the number of feet through which it is exerted. Let us see if we can determine the number of units of work performed on any mass M in imparting to it any velocity V.

It is found by experiment that the velocity acquired by a a body, when acted on by a constant force, is proportional to the time during which the force acts. Hence, since a pressure of one pound, acting for one second on one unit of mass, gives it a velocity of one foot per second, the same pressure of one pound acting on the unit of mass for a certain time, say six seconds, will give it a velocity equal to six feet per second. But we know from the laws of uniformly accelerated motion that if the body start from rest, and move by the action of a constant force, the distance passed over in the six seconds is equal to one-half the distance which would have been passed over in the same time with a uniform velocity equal to the final velocity, which is six feet per second. The distance actually passed over by the unit of mass in acquiring the velocity of six feet per second from the action of a constant pressure of one pound, is then equal to eighteen feet. The pressure of one pound has therefore been exerted through a distance of eighteen feet, and the number of units of work performed is eighteen. If the same velocity be imparted to the same unit of mass by a constant pressure of two pounds, the time employed is equal to one-half the time employed before, or three seconds; and the distance passed over is therefore nine feet, and the number of units of work is two multiplied by nine, or eighteen, the same as before. If a pressure of three pounds be used the time is two seconds, and the distance passed over is six feet, and the number of units of work is three multiplied by six, or eighteen again. And in general to impart any velocity V to one unit of mass by a constant pressure, if T be the number of seconds employed, the distance passed over will be $\frac{T\times V}{2}$ But if the pressure be one pound, the number of units in the time is equal to the number of units in the final velocity; or $T=V$. And if the pressure be equal to P pounds we shall have $T=\frac{V}{P}$ Substituting this value of T we have the distance passed over by the unit of mass,

while acquiring any velocity V, by the action of any constant pressure P, equal to $\frac{V^2}{2P}$. But the number of units of work performed by the pressure P will be equal to this distance multiplied by P; or, if W represent the number of units of work, we shall have $W=\frac{1}{2}V^2$.

This is the number of units of work performed in giving the velocity V to one unit of mass. Evidently, to give the same velocity to twice the mass, twice the work must be performed, or to give the same velocity to M units of mass, M times the work must be performed. The number of units of work, then, which must be performed by any constant pressure, on any mass M, starting from rest, to give it any velocity V, is equal to $\frac{1}{2}MV^2$; and this result is wholly independent of the time employed, the distance passed over, or the intensity of the impelling pressure, and dependent only on the number of units in the mass M and the final velocity V. We must, however, notice that if any work is performed in changing the shape of the mass, it must be in addition to the work represented by $\frac{1}{2}MV^2$.

In order to find the number of units of work performed by any constant pressure while increasing the velocity of a mass M, already having the velocity V, from this velocity to the velocity V', we have simply to observe that the work required to give the mass the velocity V' if it start from rest, is $\frac{1}{2}MV'^2$, and the work required to give it the velocity V, starting from rest is $\frac{1}{2}MV^2$; hence, the work performed while increasing the velocity from V to V', is equal to the difference between these results, or

$$\frac{1}{2}MV'^2-\frac{1}{2}MV^2=\frac{1}{2}M(V'^2-V^2).$$

We may now show that the work performed in giving the mass M, starting from rest, any velocity V, will be equal to $\frac{1}{2}MV^2$, whether the impelling pressure be constant or variable. For if it be variable we may suppose the time divided into very small periods, during each of which the pressure may be regarded as constant. Then, if, V', V'', V''', &c., be the velocities at the end of the first, second, third, &c., instants, we shall have the work performed in the first instant equal to $\frac{1}{2}MV'^2$. That in the second instant will be $\frac{1}{2}M(V''^2-V'^2)$; that in the third $\frac{1}{2}M(V'''^2-V''^2)$; and so on. Now the sum of all these elementary quantities of work is the total work performed, which is therefore

$$\frac{1}{2}MV'^2+\frac{1}{2}M(V''^2-V'^2)+\frac{1}{2}M(V'''^2-V''^2)+\ldots\ldots$$

If V be the final velocity the sum of this series will evidently be $\frac{1}{2}MV^2$, since each of the other positive terms will be cancelled by an equal negative term.

It is therefore a universal principle, that if any mass M be moved from rest, and acquire any velocity V, whether the impelling force be constant or variable, no matter what its intensity, or what the distance through which it act, or what the time employed, the number of units of work performed by the force in giving it that velocity is always the same, and is equal to $\frac{1}{2}MV^2$.

This work has been performed in overcoming the inertia of the mass; and by reason of its inertia the mass M, moving with the velocity V, has the power of performing just the same amount of work while it is coming to rest by the action of an opposing force, as was performed upon it in giving it the velocity V. For if any pressure give a mass any velocity in passing over any distance, and then cease to act, the mass, by reason of its inertia, is able to overcome an equal pressure through an equal distance, before being brought to rest, and thus to perform an equal amount of work.

The product $\frac{1}{2}MV^2$, therefore, represents the number of units of work required to impart the velocity V to the mass M, starting from rest; or the equal number of units of work which the mass M, with the velocity V, can perform before being brought to rest by any force, whether large or small, in whatever time or distance. This product, $\frac{1}{2}MV^2$, is very properly called the *vis viva*, or *living force* of the moving body, since it represents a definite power for performing work, or giving motion to other bodies; which power is exhausted, or may be said to die, at the instant the body comes to rest. The body can then impart no motion to other bodies at rest except it be first set in motion, or, as it were, brought to life again, by the action of some external power.

It should be remembered that our unit of mass here is the same as in the case

of momentum; and thus, if G be the weight of the body, the *vis viva* is equal to $\frac{1}{2}MV^2=\frac{1}{2}\frac{G}{g}V^2=\frac{GV^2}{64\frac{1}{3}}$. If we say then that a body whose mass is 10, (and weight accordingly $321\frac{2}{3}$ pounds), and whose velocity is 10 feet per second, has a *vis viva* equal to $\frac{1}{2}\times 10\times(10)^2=500$, we mean that 500 units of work must have been performed, in order to give it that velocity, starting from rest, or that it will perform 500 units of work before being brought to rest by any force whatever.

The unit of *momentum*, then, is a force of pressure equal to one pound, and the unit of *vis viva* is the *work* performed in overcoming a pressure of one pound through a distance of one foot. With this understanding there is perfect harmony in all the results. For example, if a body whose mass is 10, moving with a velocity of 10 feet per second, be brought to rest in one second by a constant force, we know that the space passed through will be 5 feet. But the constant force which can bring it to rest in one second, is a pressure equal to its *momentum*, or $10\times 10=100$ pounds. The work which the mass will perform is then $5\times 100=500$ foot-pounds, or units of work; which is equal to its *vis viva*, or $\frac{1}{2}\times 10\times(10)^2$, as it should be. Or, if the same body fall freely from rest by the action of gravity, the distance it will fall in acquiring the velocity of 10 feet per second will be 1.554 feet. The work performed on it by the force of gravity in giving it that velocity is therefore $321\frac{2}{3}\times 1.554=500$ foot-pounds again. And a like harmony may be proved true in any other special case, or may be demonstrated generally.

It should now be observed that the *momentum*, represented by MV pounds, is of no special value for estimating the power of a moving body, or anything else whatsoever with regard to its motion, any more than any other arbitrary force we may choose. For in order that MV pounds shall be a measure of the pressure the body can produce, the body must be supposed to be brought to rest without change of form in just one second of time. Thus the product MV is not a measure of the pressure a moving body is *able* to produce, or of anything else with regard to it, except under a certain arbitrary condition; the intensity of the pressure a given moving body is able to produce, while coming to rest without change of form, being wholly dependent on the condition of time or distance in which it shall be brought to rest. For if we impose the condition that the mass M, whose velocity is V, shall be so brought to rest by a constant pressure in just four seconds, it is easily shown that the distance passed over before coming to rest will be equal to $2V$. Dividing the *vis viva* of the body, or $\frac{1}{2}MV^2$ by this, we have $\frac{1}{4}MV$ for the number of pounds constant pressure the mass will exert while coming to rest. And this, or any other number of pounds, is just as valuable for measuring anything with regard to the motion of the body, as MV is, if we impose the proper condition as to the time or distance in which it shall act. And we might, of course, have deduced the *momentum*, or the constant pressure required to bring the mass M to rest from the velocity V in one second, in the same way by which we have just obtained $\frac{1}{4}MV$ as the pressure required to bring it to rest in four seconds. For if the mass be brought to rest in one second, by a constant pressure, it will move a distance equal to $\frac{1}{2}V$, and the *vis viva* divided by this gives the intensity of the pressure equal to MV. This simple process would have been sufficient to show precisely the meaning of the product MV; but there is such a tendency to consider that product as in some way representing a *quantity of motion*, that it seems advantageous to establish its exact signification by two independent methods.

The product $\frac{1}{2}MV^2$, rightly understood, is, on the contrary, a valuable and definite measure of the accumulated energy of the moving body, without any arbitrary conditions whatever; and, as before remarked, is very appropriately named the *vis viva* of the body.

II. MOTION PRODUCED BY IMPACT.

We will now discuss the relation of momentum and vis viva to the case in which motion is communicated by impact from a moving body to another body previously at rest, both the bodies being supposed to be inelastic. In the case of one inelastic body in motion

striking another inelastic body at rest, with its center of gravity in the line of direction of that of the moving body, so that after impact the two bodies shall move on together as one body, the ordinary mode of obtaining the velocity of the combined mass is by assuming that the momentum of the whole after impact will be equal to the momentum of the first body before impact. Thus, if M be the mass of the moving body, and V its velocity, M' the mass of the body at rest, and V' the velocity of the combined mass after impact, it is assumed that after impact we shall have $(M+M')V' = MV$, whence $V' = V\frac{M}{M+M'}$. The argument by which such an equality is inferred is given as follows in "Cooke's Chemical Physics," a work of deservedly high reputation:

"We shall consider the bodies as completely devoid of elasticity, and so constituted that after the collision they shall move as one body. Let us then inquire what will be the direction and velocity of the united mass after the impact. The mass M', being previously at rest, can have no motion save what it may receive from the mass M, and consequently must move in the same direction as the mass M moved in before the collision. Again, since bodies cannot generate or destroy motion in themselves, it follows that whatever motion the mass M' may acquire must be lost by the mass M; and also, that the total momentum of the united masses after the collision must be exactly equal to the momentum of the mass M before it."

In the work from which this is quoted, the true meaning of momentum, as previously explained, had once been given. But it seems to have been lost sight of in this argument. For it is here implied that momentum is either the supposed quantity of motion of a moving body, or at least proportional to such a quantity. If this were so, and if there were no modifying circumstance overlooked, the argument might be valid. But is momentum proportional to any such quantity? And is there no important modifying circumstance unnoticed? Let us examine it a little.

In the former article we arrived at a definite understanding of the meaning of the terms *momentum* and *vis viva*. We saw that *momentum*, which is equal to MV, represents the number of pounds pressure which the mass M with the velocity V is capable of exerting under a certain arbitrary condition; and that the *vis viva*, which is equal to $\frac{1}{2}MV^2$, represents the number of foot-pounds of work which the mass M with the velocity V must perform before being brought to rest by any force, constant or variable, without any conditions as to time, or the intensity of the force. Accordingly, if the mass M have its velocity reduced by any force, from the velocity V to a less velocity V', the work which the mass must perform during that reduction will be a definite quantity, viz:

$$\frac{1}{2}MV^2 - \frac{1}{2}MV'^2 = \frac{1}{2}M(V^2 - V'^2).$$

If, then, the velocity of any moving body be reduced by impact upon a body at rest, and the two move on as one, a certain definite amount of work must be performed by the first body during the impact, by reason of the reduction of its velocity. Now, since any body is capable of performing, before coming to rest from any velocity, the same amount of work as is expended in giving it that velocity, it would follow that in case of a mass M with the velocity V impinging on another mass M' at rest, so that the two should move on with the common velocity V', if all the work performed by the mass M, by reason of the reduction of its velocity from V to V', were employed in imparting the velocity V' to the mass M', the work which the combined mass after impact could perform before being brought to rest would be the same as that which the first body was capable of performing before impact. We should thus have the *vis viva* after impact equal to the *vis viva* before impact, or, $\frac{1}{2}(M+M')V'^2 = \frac{1}{2}MV^2$; from which we should derive $V' = V\sqrt{\frac{M}{M+M'}}$.

We arrive at this result by a logical argument from known facts and principles; but only under a hypothesis which is not true in fact, viz.: that all the work performed by reason of the reduction of velocity of the one body, be employed in imparting velocity to the other. In fact, whenever impact between two bodies occurs there is always more or less change of form, or compression of

the mass of both bodies, either permanent or temporary; and a certain amount of work is employed in producing this compression. If the bodies are inelastic they remain permanently compressed, and in such cases there is always a certain amount of heat developed, which is the exact equivalent of the work performed in producing the compression. That is to say, ordinary motion through space is converted into molecular motion. And it is evident that if a body in motion strike a body at rest, none of the work which the moving body performs in the development of heat, or molecular motion, can at the same time be employed in imparting ordinary motion to the other body. Hence, in the impact of inelastic bodies, not all the work performed in the reduction of the velocity of the one can be employed in imparting velocity to the other; therefore, in the case supposed, V' must be less than

$$V\sqrt{\frac{M}{M+M'}},$$ instead of equal to it.

And we now see a very important modifying circumstance entirely unnoticed in the argument above quoted from Cooke. The argument says nothing about any destruction of ordinary motion, during impact, by its conversion into heat. On the contrary, the argument states that "bodies cannot generate or destroy motion in themselves;" which, though it may be true in the widest sense, is not applicable here when considered alone with reference to ordinary motion. For the bodies are supposed to be completely inelastic; and we know that in every case of impact of inelastic substances, heat or molecular motion is developed, and developed only by the destruction or conversion of ordinary motion. If, then, momentum, or MV, really represented or were proportional to a hypothetical quantity of ordinary motion, which is the only kind of motion it could in any way be conceived as representing, the momentum of the combined mass after impact, or $(M+M')V'$, could not possibly be equal to that of the body in motion before impact, or MV; since in every case of impact the velocity of the moving body will be partially destroyed by the conversion of ordinary motion into heat, or molecular motion, which the product $(M+M')V'$ could in no way include.

In what has now been said attention is simply called to the fact that the usual argument for deriving the equation, $(M+M')V'=MV$, does not give satisfactory proof of it. It has not been denied that the equation may be true. For if momentum, or MV, does not represent a quantity of ordinary motion, (whatever that be), and is not proportional to such a quantity, it may then be possible that after impact in the case considered, even though there be a conversion of ordinary motion into heat, $(M+M')V'$ will be equal to MV. If it be a true equation we have $V'=V\frac{M}{M+M'}$. But the velocity previously deduced under the supposition that there be no loss of vis viva is $V'=V\sqrt{\frac{M}{M+M'}}$. Hence, if the first of these equations be true it leads to the result that whatever the velocity of the moving body, or whatever the ratio of the masses, the loss of vis viva by the impact, or the work performed in producing molecular motion, is just sufficient to account for the reduction of the velocity of the combined mass after impact, from $V\sqrt{\frac{M}{M+M'}}$ to $V\frac{M}{M+M'}$. But have we any right to make an assumption which leads to such a result, without a demonstration of the proportion of work consumed in the development of molecular motion? or without a hint that there is any work so consumed? Although some of the books notice that the formulas as ordinarily deduced involve a loss of vis viva by impact, none of them, so far as observed, attempt to explain what that lost vis viva has been doing, or whether it has done anything. We are left to infer that a part of the work, which previous reasoning shows must be performed in any reduction of velocity, has been doing nothing. Vis viva has mysteriously disappeared, and that is all.

If then we are not at liberty to assume that in the case of impact here considered either the vis viva or the momentum after impact will be equal to that before impact, how can we correctly obtain the

common velocity of the two bodies after impact? The following method is thought to be free from the objections brought against the usual argument.

We know that the velocities generated or destroyed in two masses, by the respective actions for the same time of two forces of equal intensity, are inversely proportional to the masses. In the case of impact, although the time of the action may be very short, there is nevertheless a certain amount of time spent before the velocity of the two masses can become the same; and the acting forces are subject to the same laws that govern in all other cases. Now, when one body at rest is struck by another body in motion, however the pressure may vary in intensity, or whatever may be the molecular disturbances of action and reaction produced, there will at every instant before the velocity of the two masses becomes the same be a total resultant pressure on the mass of the first body, in the direction of the motion. And by the principle that action and reaction are always equal and opposite, the resultant pressure on the mass of the second body must be all the time equal to that on the first, and in the direction opposite to its motion. The velocity generated in each instant in the first body and that destroyed in the second body will then be to each other inversely as the masses. This being true for each instant, must be true for the whole time of the action; so that the total velocity generated in the first body by the impact will be to that destroyed in the second body inversely as the masses. If M be the mass of the moving body, and M' that of the body at rest, V the velocity of the mass M, and V' their common velocity after impact, then $(V-V')$ will represent the velocity lost by M in the impact, and V' will represent the velocity imparted to M'. We shall thus have the proportion $M : M' :: V' : (V-V')$. From which we derive $V'=V\frac{M}{M+M'}$.

The velocity, V', is the common velocity of the two bodies at the instant their velocities have become equal. If the bodies were perfectly inelastic there would then be no tendency for them to separate, and neither of them would have any further power to change the velocity of the other. They would therefore move on together with the common velocity.

This velocity, $V'=V\frac{M}{M+M'}$, has now been deduced from a consideration of the acting forces, without any assumption as to the vis viva or momentum after impact. We may therefore have confidence that it is correct; and experiments with bodies so arranged as to be obliged to move together after impact show it to be the true velocity in the case considered. Knowing it to be true, and knowing also that if there were no loss of vis viva from the production of molecular motion the velocity of the combined mass would be $V'=V\sqrt{\frac{M}{M+M'}}$, we may feel sure that the difference between these velocities is due to the conversion of ordinary motion into molecular motion, or heat, and there need be no mystery as to what has become of the lost vis viva.

Let us now determine the loss of vis viva occasioned by the impact, and we shall thus have the mechanical equivalent of the heat developed. The vis viva of the mass M before impact is equal to $\frac{1}{2}MV^2$. That of the combined mass after impact is

$$\tfrac{1}{2}(M+M')\frac{M^2V^2}{(M+M')^2}=\tfrac{1}{2}MV^2\frac{M}{M+M'}$$

The difference between them is

$$\tfrac{1}{2}MV^2 - \tfrac{1}{2}MV^2\frac{M}{M+M'}=\tfrac{1}{2}MV^2\times\frac{M'}{M+M'}.$$

The number of units then in this expression, $\frac{1}{2}MV^2\times\frac{M'}{M+M'}$, is the number of foot-pounds of mechanical work which, in the impact of bodies in the way supposed, is employed in the production of heat. And we see that with a given velocity of the mass M, the amount of work employed in the production of heat varies with $\frac{M'}{M+M'}$, or the ratio of the mass previously at rest to the combined mass after impact. Thus, if M and M' are equal, we have this ratio equal to $\frac{1}{2}$. That is, one-half the vis viva of the mass M before impact is employed during impact in imparting velocity to the mass

M', and the other half in the development of heat. If M' is very small compared with M, there will be hardly any loss of vis viva, from the production of heat. If M' is very large compared with M, nearly all the vis viva of the latter previous to impact will be consumed during impact in the production of heat. In the case of a small mass M falling upon the earth, for instance, its mass will be practically infinitesimal in comparison with that of M' in this ratio, $\frac{M'}{M+M'}$, which then differs from 1 only by an infinitesimal quantity. In this case all but an infinitesimal part of the vis viva of the mass M before impact is by the impact converted into heat; which we know is practically true when the motion of a body is suddenly arrested.

After having thus determined the loss of vis viva by the impact of two inelastic bodies, we may next proceed to compute the rise of temperature which the combined mass will undergo by the impact, supposing the heat to be uniformly distributed. We know that the expenditure of 772 foot-pounds of work is capable of raising the temperature of a pound of water one degree Fahrenheit. The relative capacity for heat, of different substances, has been determined and tabulated; and from these data, knowing the material of the bodies between which impact occurs, and the number of foot-pounds of work converted into heat, as above determined, we can easily calculate the rise of temperature under the condition supposed.

Let us now examine the momentum after impact. From the equation $V' = V\frac{M}{M+M'}$, or from the proportion by which it was obtained, we have $(M+M')V' = MV$; which proves that the momentum, or constant opposing pressure required to bring the combined mass to rest in one second from the new velocity V', is equal to that which would have been required to bring the mass M to rest in an equal time from the velocity V. But this does not prove momentum to be a quantity of motion, as might once have been thought. On the contrary, it proves most conclusively that it cannot be any such thing. For we know there has been change from ordinary motion to molecular motion during the impact, and yet the momentum remains unaltered.

There is then a compensation of errors in the ordinary argument for obtaining the velocity of the mass after impact; the first being the assumption that momentum can in some way represent or be proportional to the supposed quantity of motion of the moving body, and the second being another assumption that none of the motion supposed to be represented by the momentum can be lost by the impact; thus making it appear correct to place the momentum after impact equal to that before impact. And the fact that experiment confirms the truth of the equation, has probably prevented a clear view of the errors involved in the argument by which it was derived.

Ever since the time of Leibnitz there has been more or less discussion among mathematicians and philosophers as to whether momentum or vis viva ought to be taken as the measure of the quantity of motion of a moving body. And it is not difficult to see how the fact that momentum after impact of inelastic bodies is equal to that before it, might, without a clear knowledge of the laws of the correlation of forces, leave some chance for such discussion. The greater wonder is that after those laws have been so long established we still persist in defining momentum as a quantity of motion. Knowing that in every case of impact as here considered more or less change takes place from ordinary motion to molecular motion, and knowing also that after impact the product of the combined mass into its velocity is equal to the product of the mass and velocity of the body moving before impact, how can we possibly admit that those products can in any way represent or be proportional to the supposed quantity of motion in the two cases? And yet, momentum is defined as a quantity or amount of motion, in our best dictionaries, general and mechanical; in such works as Bartlett's Analytical Mechanics, Peck's Elements of Mechanics, Silliman's Physics, and in most if not all the popular works on Natural Philosophy.*

May it not also be that a great part of the obscurity of this subject is owing to

* Cooke's Chemical Physics does not so define it, although using the argument before quoted.

some logical absurdity in the very expression *quantity of motion?* Is not motion essentially a *condition* of bodies, rather than a *quantity* of anything which they can possess? Is it proper to say that one body has two or three times as much of that condition as another body? It is correct to speak of the *rate* of motion, or velocity of one body, as being twice as great as that of another, for their velocities can be compared in terms of the same unit. It is also correct to speak of the accumulated energy for overcoming and moving against resistance, or producing motion in other bodies, which a moving body possesses by virtue of its inertia, as being twice as great as that of another body; for here again comparison may be made in terms of a common unit, viz. the foot-pound. And this energy we know to be represented by the vis viva. What need or possibility is there of a *quantity of motion* different from one or both of these? And if there can be such a quantity, what is its unit?

The compound unit of vis viva is admissible, because a pressure of one pound exerted through a distance of one foot can be made to exert, and does exert in nature, the same pressure through the same distance, or if the pressure of one pound for a distance of one foot be neutralized by a pressure of one-half a pound the latter must be exerted through a distance of two feet; or if the pressure of one pound for a distance of one foot be neutralized by a pressure of two pounds the latter will be exerted through only one-half a foot; and so on, the product of the pressure multiplied by the distance through which that pressure acts, in neutralizing a pressure of one pound for a distance of one foot, being always equal to unity. But if we were to compare the motion of two bodies with different velocities, and assume that their quantities of motion could have such a complex unit as a unit of mass moving through a unit of distance in a unit of time, we should find it inadmissible in fact. For if a body could have a quantity of motion it ought certainly to be equal to that which the body could impart to another body by coming to rest itself, supposing there were no loss from the production of molecular motion in the transfer. But a unit of mass with one unit of velocity cannot possibly by coming to rest impart a double velocity to one-half the unit of mass. And if all the work which a unit of mass with a unit of velocity could possibly perform while coming to rest were employed in imparting velocity to one-half a unit of mass, its velocity would only be equal to $\sqrt{2}$. Whereas, if all that work were performed in imparting velocity to a double mass the velocity produced would be equal to $\frac{1}{\sqrt{2}}$. In neither case would the product of the mass acted on, multiplied by the velocity imparted, be equal to unity. The supposed unit could not then be admitted as a true standard of comparison of quantity of motion. Is it then possible to find a proper unit for such a quantity? And if not, is it not absurd to speak of a quantity that has no conceivable unit?

Should we not at all events restrict the word momentum, as applied to the product MV, to its proper and demonstrable meaning, or else discard it altogether? In fact that product is hardly more worthy of a special name than $\frac{1}{2}MV$, or any other fraction of MV, since it measures nothing except under the condition of acting for one second of time; and in practical cases it would be difficult, if not impossible, to apply a constant opposing force of just that intensity to any moving body. The fact that momentum before and after impact is the same, is not of so much value as the more general truth that the constant pressure which the combined mass after impact could exert while coming to rest in *any time*, is equal to that which would have been exerted by the body previously in motion, if brought to rest by a constant force in an equal time. This may easily be demonstrated by dividing the equation between the momenta by the number of seconds in the time. Each member of the equation will then represent the number of pounds pressure required to bring the respective masses to rest from their respective velocities, in the given time. And this fact may be worth knowing, although the work which the combined mass can perform after impact will be less than that which the body first moving was capable of performing, in proportion as the velocity is less.

NOTE ON "RECENT EXPERIMENTS ON FLOW OF WATER IN RIVERS AND CANALS."

By CLEMENS HERSCHEL, C.E., of Boston.

Written for Van Nostrand's Engineering Magazine.

We are told that the first time a diversity of language was shown to be very conducive to misunderstandings and other unpleasantness among men, was at the building of the Tower of Babel, and the experience of the ages that have passed since then, have presented some notable examples of the same causes, producing the same effect, so also in our own day and in our own limited circle of acquaintance.

To illustrate: I have been a diligent reader of the professional literature of this country, of England, France, and Germany, for the past fifteen or twenty years, and can recall an almost continuous series of such instances. Perhaps the latest, and a much to be regretted example, is the one in the last number of this Magazine, in which General H. L. Abbot gives expression to what would be righteous indignation, were the cause of it correctly represented, which it is not. In view of the eminent standing, professional and official, of all the parties concerned, no less than from a general desire to act in the interests of peace and good will, I have thought it proper to endeavor to clear up some misunderstandings; also to say a word to American readers, that may lead them to honor and respect the name of his Excellency, Dr. G. Hagen,* late (Ober-landes-bau-director) chief of the engineering department of the kingdom of Prussia and its provinces, as it is honored and respected by probably all who have ever read his works, or who know of his eminent services.

Gen. Abbot writes that the remark that Humphreys and Abbot's "results were altered in part to establish" (accommodate themselves to, or to fit, would have been a better translation)—to fit "the theory proposed," "implies a dishonest tampering with the field-notes—a suggestion which, whether made by Mr. Bornemann or Mr. Hagen, or by any other person is absolutely false and libelous." Such, however, is not my understanding of the phrase quoted, especially not in the original, where the word "data" would have been used in the German, instead of "results," had the charge of changing field-notes been implied or brought forward. But Dr. Hagen is not the man to bring any charges lightly or in a wrong spirit, and I shall endeavor to show now, just what he has said on the subject matter. I pass by anything said by Mr. Bornemann, for he clearly refers only to what had already been said by Dr. Hagen, and does not add anything new.

Dr. Hagen first wrote about the book—"The Physics and Hydraulics of the Mississippi River," in November, 1867, until which time, as he himself says, he had been unable to get a copy of the work in the original, and did not wish to use the translation. (See Z. fur Bauwesen, 1868, p. 63.) The parts of this work that have most attracted the attention of German hydraulic engineers, are the one that treats of the relations between the slope of a river, its hydraulic mean depth, and its mean velocity, and the one that describes the curve or scale of velocities in any vertical. Dr. Hagen does not accept, as *the final and true general law, applicable to all streams*, either of the formulae proposed, as representing these two relations. He does not think that these are the best formulae for that purpose that will ever be found, nor as good

* Dr. Hagen must be now nearly, or a little more than, eighty years of age. In his youth he was a pupil of the celebrated astronomer Bessel; passing from this study to that of engineering, more especially of hydraulic engineering, it has been his life-work to introduce into the practice and study of the engineering profession the same accuracy, logical methods and careful weighing of scientific testimony, that have enabled astronomy to take the high rank she holds among the sciences. He must have been fully fifty years in the Prussia State service, and finally attained the position and rank above mentioned, that is, Chief of the whole Engineering Service of the kingdom, a position analogous to that of general of the army—the U. S. Army, and equally honorable. Upon his retirement from active service last year, the Emperor of Germany, by special decree, conferred upon him the title of "Excellency." That his mind has lost nothing of its original vigor, is plainly to be seen from his last work, "Researches on the Uniform Flow of Water," Berlin, 1876, containing a digest of all that has been done in this line of study to date, and offering to show, not so much a solution of the question, as the way in and by which the perfect answer, if at all attainable, is to be found in the future.

as can even now be proposed, and in fact, somewhat discourages the idea, by his later writings, of there being any one set of formulae hidden in Nature that will fit all streams large or small, steep and of gentle inclination, with equal accuracy. "But," says he, in the article above mentioned, "if these supposed theories are not to be accepted as such, this will nevertheless cast no doubts upon the observations (the field-notes) themselves; on the contrary, we must conclude from the lack of coincidence of these latter with the former, that they have been communicated to us, completely and without change. It can also not fail to strike the reader, from the description given of the methods employed, that the observations were conducted with thought, and in an expert manner. They rank unequivocably among the best hydrometric observations that we possess, and the circumstance of their having been made upon so mighty a river as the Mississippi, only gives them the greater value. But it is to be regretted that the observations (alluding now to those taken for determining the curve of vertical velocities, pp. 230–232, Humphreys and Abbot's book) were not printed as made, but are recorded by averages; also that some details were not described, which, nevertheless, are by no means without importance."

I think that enough has been said to prove that neither Dr. Hagen nor Mr. Bornemann have ever as much as intimated that the field-notes of the Mississippi Survey have been tampered with; as it happens they have asserted, on the contrary, that this evidently had not taken place. And as the remarks, whose misconstruction has led to the misunderstanding here spoken of, occur in the discussion of the curve of velocities in a vertical, and my present object is only to remove this misunderstanding, I shall have to confine myself to this one part of Dr. Hagen's review, however interesting and useful to any hydraulic engineer it might be, to get Dr. Hagen's views on the question of the mean velocity of a river also. In fact, the whole work mentioned in the foot-note ought to be translated into all the principal languages, as a standard guide for the profession, and so as to be within reach of hydraulic engineers the world over. But this is not the place to attempt anything of the sort, in however abbreviated a manner it might be done. I cannot even reproduce here, in translation, the whole of Dr. Hagen's discussion of the experiments, and the method of reducing them, that led to the finding of the curve showing the scale of velocities in a vertical, and its equation, as shown pp. 230–234, of Humphreys and Abbot's book. It occupies three quarto pages in the German. Suffice it to say, that Dr. Hagen gives good reasons for rejecting, first, the *methods* used for *reducing* the observations (which lead to what are called in the German the "results" of the observations); and secondly, these results themselves, as given by the parabolic curve and its equation.

After rejecting, further, any hypothesis, that certain observations were suppressed, or simply *not used*, because they did not agree with the theory proposed (a very mild form of sin in scientific research), and not even intimating the least suspicion of such a thing as altering a record once made (a gross form of sin in any sort of business), Dr. Hagen goes on and says (p. 69 of the article of 1867, above referred to): "There is, however, another and very probable explanation of this (remarkable) coincidence (of theory with the observations). Let any one try to draw curves of the described properties, so as to fit the plotted observations above given, and he will find that this procedure is one admitting of the greatest latitude. (The irregular observations shown in the plot are: the 4th, the series of Group I; 2d, series of Group II; and 3d, series of Group III; pp. 230–232, Physics and Hydr. Miss. River). Very dissimilar curves have all an equal validity. Each curve may be changed throughout its length, or only in part, without introducing any error. In this way it became an easy matter, to cause the observations to fit a presupposed form of curve within any desired degree of accuracy. It would not even have been difficult, to draw the curves in such way, that the mean values would have coincided with the calculation to seven places of decimals or closer still." And similar language, together with a reference to his previous writings, is used in Dr. Hagen's book of 1876. It is to one or all of these that Mr. Bornemann

has referred, so that the remark in this Magazine, quoted at the beginning of this note, and replied to by Gen. Abbot, has reference to a little piece of self-deception, practiced by those who *reduced* the observations, upon themselves, and caused by using curves plotted by the eye, with only a limited number of points for a guide, as a means of deducing laws from observations, where the employment, in a rigorous manner, of the rigorous method of "least squares," would have avoided this difficulty, and in nowise referring to so much as a suspicion of any changes made in the field-notes.

Finally, I trust that my efforts will not prove the starting point for new misunderstandings; but if so, that their evil effects may be visited upon me alone. I feel that Dr. Hagen has had but a poor apologist in me; his own writings, in their entirety, are his best defence. It has been an ever recurring pleasure to me, for years, to read his writings, as they come from his pen, and I only wish that the honest, conscientious search after the truth, modesty of spirit, and clear conciseness of language which they contain, may some day be made accessible to the English reader, by a good translation.

INDIAN IRRIGATION WORKS.

From "The Engineer."

No one can look upon the vast remains of ancient works of irrigation in India and Ceylon without pondering over the causes which have led to their isolation in uninhabited tracts of country, where wild animals roam at will undisturbed by the presence of man. The question naturally suggests itself as to the object of its construction in such localities, and as to the cause of their total abandoment. It is not a question, however, which need remain long unanswered, for ancient records prove that at the time these works were executed these now desolate regions teemed with human life. The autocrats by whose will these great failures were carried out little reckoned of the cost at which such monuments had to be purchased. In their shortsightedness they saw nothing but the capabilities of the country for producing the staple food, rice, and altogether ignored those sanitary and other conditions which were involved in the massing together of great bodies of forced labor, unregulated by any system which should preserve them in health. Whole districts were, at the simple word of the satrap, laid under contribution for labor, and the large number of people collected were left to shift for themselves, unprovided with shelter or means of cleanliness, and almost without food. At least, it is certain that the latter necessary was doled out with such a restricting hand that the poor wretches, thus forced to hard and continuous labor, were scarcely kept alive by the pittance of food supplied them. The natural consequence of such shortsightedness followed. Imagine probably some twenty thousand men, women, and children huddled together under such conditions, and who can feel surprised that disease broke out among them to such an extent that, spreading from them as a centre, whole provinces became infected, and death swept off its tens of thousands of victims, until a country, once densely populated and carefully cultivated, became an arid, uninhabited waste, and the vast works which were designed to stimulate production were left to go to ruin uncared for and untended.

The size of these ancient works may be conceived from a few prominent examples. The Poonary tank, in the Trichinopoly district, is thirty miles in length, that of Weeranum ten, whilst the Giant's tank in Ceylon has a diameter of about twelve miles. Many of the Bunds constructed to secure these vast inland sheets of water were 40 feet high in places, and the size and girth of the trees which now cover them testify to the many centuries which have passed since their erection, and where nothing is now heard but the roar of the elephant

and the sharp, short bark of the leopard. It is in the attempt to re-utilise these vast works of a bygone age that our Government has committed, we consider, one of their leading mistakes. There was the land, and there, with large outlay, would again be the water to irrigate it; but where was the population to cultivate it? It was conceived, and conceived erroneously, that given these advantages, the natives would leave those districts which were overcrowded by a teeming population and emigrate to avail themselves of them. But no being on earth is so conservative in his prejudices as the native of India. Generation after generation squat down under the vines and fig-trees of their forefathers, and their they would rather starve and die from diseases generated from over-crowding than leave the place where they were born. Consequently large sums have been thus fruitlessly expended which might, with a more judicious forethought—which needed scarcely, one would think, the teaching of sad experience—have been laid out upon the improvement and development of smaller works in localities where there already existed a nucleus of population. Noticeable instances of such failure are to be found in the large works at Rukam and Kanthali, in the eastern provinces of Ceylon, where many tens of thousands of pounds have been expended in their restoration, only to find the lands irrigated by them still untenanted, and with but little prospect of the Government ever being recouped its outlay by their sale. Steady application is a rare quality among many of the Indian castes, and they prefer, to the laborious occupation of rice cultivation, the ruinous practice of chena-ing—that is to say, the felling of a few acres of old forest, and the rapid exhaustion of the land by successive crops of dry grain, such land being abandoned when the soil has been made to yield until perfectly sterilized. To such an injurious extent has this practice prevailed, that whole districts have become denuded of forests, and many years must pass before the land so exhausted will produce anything beyond a miserable stunted jungle. The authorities have at last become awakened to the impolicy of permitting such proceedings, which would result in an ultimate scarcity of timber, and the still further depopulation of the country.

The mistakes above described are now pretty generally admitted, and more is being done to foster cultivation in peopled districts by small but judicious outlay on existing used works. In no way can expenditure be better applied than in improving and regulating the distribution of the water contained in the small tanks with which nearly every village is provided. From time immemorial the roughest means have been used for such distribution, and waste and litigation between the village populations have resulted. The exit for the water was usually of the roughest description, a well being sunk in the centre of the "bund," having tunnels below it to either foot of the base of the embankment; and the stoppage of the effluent water was effected by sinking in the well bundles of boughs tied up into a sort of rough fascine. How ineffective such means were under the pressure of a head of water often from 20 ft. to 30 ft., may be readily imagined. Again, when the water passed into the main distributing channels, the only method of its admeasurement to the several lands was by a log of wood crossing the channel, just above the surface level of the water, in which were cut notches, through which only it was able to flow, and the size of such notches bore a relative correspondence to the area of land to be cultivated by each proprietor. To remedy this state of things, amongst other innovators on the old system, Mr. G. L. Molesworth, now chief engineer of the State Railways in India, proposed to the Government of Ceylon the erection of small head and distributing sluices, having iron paddles fitted in wooden frames, which can be readily fixed by almost unskilled labor for a trifling outlay, which has, in every case, been soon recouped to the villagers by the saving effected. It is to such simple means as these that we desire to see the attention of Government directed —not to the total exclusion of more ambitious works, but for the fostering of the economical use of the supply now available to existing communities.

A further mistake is, in our opinion, committed by the over-strict adherence by irrigation engineers to the old methods of native construction. As long as

brickwork is confined to foundations, and not exposed to the ravishes of the prolific vegetation of a tropical climate, it is scarcely possible to devise more simple or more efficient work; but it is a different thing when we come to consider the common use of brickwork in the superstructure of dams. The seed of the banian or Indian fig, is probably the most dangerous enemy to such construction, and once let it obtain a foothold in some unfilled joint or cracked plastering, and the destruction of the dam is simply a matter of time and that not of long duration. It is urged on behalf of using brick that a little care exercised—in fact, a stich in time—will prevent disastrous consequences; but no one acquainted with the *laissez aller* frame of mind of an Indian native would like to guarantee such watchfulness. Once let the seed develop, and rapidly a mighty force begins to exert itself, and the parasite increases in bulk, with a speed unknown in more temperate regions, and rends the work from top to bottom. We hold, therefore, that when conditions may admit—and they do so, in our opinion, in the majority of cases—brickwork should not be used in such erections, and even heavy masonry should be avoided when more durable means can be employed. Often have we seen huge solid masses of granite split from top to bottom by the growth of this insidious plant, and there is scarcely a piece of ancient masonry existing which has not been rendered valueless for useful purposes from this cause. We would suggest the freer use of iron in these works when they can be maintained under proper supervision. The use of this material has so largely extended for coffer-dams and other kindred purposes that there is little to be urged against its adoption as we propose, and if Professor Barff's new process can in time be made applicable to moderate sized castings, there is no reason why dams should not be formed of it which would be almost imperishable, whilst the foundations for such superstructure would be much less in extent than are now required. Concrete, too, might be far more used in the form of pisé work in such constructions. Its greater solidity would afford less opportunity for the growth of parasitic plants, and the excellent quality of the Indian limes would make this material especially adaptable. The method by which the Government now recoups itself from the cultivators by annual water rates is, among a people possessing the characteristics of the Indian native, open to many objections. Far from regarding it in its true light as payment for value received, the ryot looks upon it as a tax, and specially obnoxious to them, as are all direct taxes, a prejudice against which has rendered the adjustment of the Indian revenue at all times a matter of great difficulty. It is not easy, however, to point out a means by which this prejudice, which has largely militated against the value of these works, can be overcome in the case of irrigation being applied to lands already in native possession; but we do think it is a feasible suggestion, and one which would greatly tend to remove the difficulty of the present system, if all Government land sold in connection with such works were to have an upset price put upon them, which should include the value of the privilege of water supply. At present, such lands are sold under condition of payment by annual instalments, and the rate added for capitilisation of the annual payment would not, we consider, be severely felt; whilst the investor would feel that his land had the fully increased value, and as being held under a more unconditional and independent title. Colonel Arthur Cotton, whose name must ever be associated most honorably with the endeavor to extend the blessing of irrigation to the people of India, we cannot but consider to have weakened his cause, and led up to many disappointing results by his insistance on the superior merits of irrigation canals in India, as a means of transport. The result of such insistence has been in many instances, a sacrificing of the immediate object of irrigation, the success of which was guarranteed by known facts, to a prospective and visionary advantage; the fallacy of such anticipations having now been demonstrated by sad experience. It is but in few cases that such works, having combined objects, have proved remunerative, as they have generally been carried out on too magnificent a scale. Let our authorities take warning by the impressive lesson taught in the decayed grandeur of the

attemps of past ages; and, whilst we rejoice to hear of instances such as are given in the last report of the Madras Irrigation and Canal Company, whose works last year irrigated no less than 90,750 acres, we trust attention will be paid to aiding more fully than hitherto the wants of small detached village communities, which have before been comparatively neglected for the prosecution of grand, but from a monetary point of view, unsuccessful, schemes.

STEAM BOILERS AND ENGINES FOR HIGH PRESSURES.*

By Mr. LOFTUS PERKINS, of London.

From "Engineering."

The object of this paper is to bring before the Institution plans for generating high-pressure steam, say from 250 lbs. to 1000 lbs. per square inch, and working it with great expansion and perfect safety, in conjunction with simplicity and durability. Sixteen years ago the author, conjointly with Professor Williamson, read a paper on this subject at a meeting of this Institution in 1861. The engine and boiler then described have been in use ever since, and recently became the property of a gentleman who for several years has had another boiler and engine on the same system at work. The boiler and engine of 1861 are to be re-erected at the new works of the Sub-Wealden Gypsum Company, at Battle, near Hastings, and are to form part of a steam plant consisting of three sets of boilers and engines, &c., on this system. Since 1861 many improvements have been effected, and are embodied in the engines recently constructed.

In generating steam of the high pressure required to realize a fuller benefit of expansion, it has previously been found impossible to combine in the boiler great strength and safety with durability; if the former are secured, by reducing the internal dimensions and capacity of the boiler, the impurities passed in are fatal to the latter. In working a marine engine which was designed to use water distilled from sea-water, the author found that, although extreme care was taken to separate all the impurities from it before it was introduced into the boiler, the internal surfaces were in the course of time seriously injured. In the same manner, ordinary marine boilers using surface condensation have been injured when there has been an insufficient supply of sea-water to form a protecting scale on the exposed internal surfaces. This led the author to seek for a remedy, which he succeeded in discovering, and adopted with absolute success. This was the use of nothing but fresh water, or distilled fresh water in the boiler, used over and over again, without any admixture of sea-water or the products of seawater, and this was easily accomplished, as the machinery in question had been designed to avoid any leak whatever, and the amount of waste that did take place from glands, &c., was so small in quantity, that no practical inconvenience was found in providing the small supply of fresh water required to make good the waste that did occur.

The means taken to secure the soundness of all the joints and parts of the machinery were the same as those which had previously proved successful in the manufacture of the high-pressure heating apparatus which the author and his firm have been making for upwards of forty-five years, and which has continued to work with the same water with which it was originally charged, without any destructive effect on the internal surfaces. Many sets of this heating apparatus have been working forty years without decay; and some specimens of tubes from the boiler that was described in the former paper in 1861, which were cut out of this boiler for the Admiralty Boiler Committee in 1874, were found to be in such a remarkably good state of preservation that the committee made a special report on the system, which was laid before Parliament, and the specimens referred to are now shown on the

* Paper read before the Institution of Mechanical Engineers

table by the kind permission of the committee. The committee examined the condition of the boiler and cylinders of the engine at the writer's works, which were opened for the purpose, in the presence of the committee; and found the tubes of the boiler in a remarkably good state of preservation after having been in use nearly thirteen years, and the piston packing and valve rings made of the special metal were found in excellent condition after eighteen months' working without lubrication since last examined.

The possibility of using water which did not injure the internal surface of the boiler enabled the author to design the boiler on a system that combines maximum strength and safety. The horizontal tubes are $2\frac{1}{4}$ in. internal and 3 in. external diameter, excepting the steam collecting tube, which is 4 in. internal and $5\frac{1}{2}$ in. external diameter. The horizontal tubes are welded up at each end $\frac{1}{2}$ inch thick, and connected by small vertical tubes of $\frac{7}{8}$ in. internal and $1\frac{5}{16}$ in. external diameter. The firebox is formed of tubes bent into a rectangular shape placed $1\frac{3}{4}$ in. apart, and connected by numerous small vertical tubes $\frac{7}{8}$ in. internal diameter. The body of the boiler is made of a number of vertical sections, composed each of eleven tubes, connected at each end by a vertical one; these sections are connected at both ends by a vertical tube to the top ring of the firebox, and by another to the steam collecting tube. The whole of the boiler is surrounded by a double casing of thin sheet iron, filled up with vegetable black to avoid loss of heat. Every tube is separately proved by hydraulic pressure to 4000 lbs. per square inch, and the boiler in its complete state to 2000 lbs., this pressure remaining in some hours without showing any signs of leakage. Experience of a very extensive character has proved that this construction of boiler can be worked safely, with great regularity, and without priming, and that the steam produced is remarkable for its freedom from moisture. The area through the vertical connecting tubes is found ample for allowing of the free escape of the steam, and for the prevention of injury from overheating of the tubes in contact with the flame. Injury arising from a prolonged stoppage of the feed supply is a casualty to which all boilers are liable, but with this construction of boiler the small capacity of the sections reduces to a minimum any danger arising from such injury, and facilitates rapidity of repair.

The engine has three cylinders; the first is a single acting high-pressure cylinder, and the second also a single acting cylinder, four times the capacity of the first; these two cylinders are bolted together in the same straight line, and have a common piston rod. The third cylinder is double acting, four times the capacity of the second, and its piston-rod is connected to a crank at right angles to the other crank.

Having safely generated steam of high pressure at say 350 lbs. per square inch, a serious difficulty has to be overcome in using it, from the high temperature affecting the lubrication of the pistons and packing of the glands. This difficulty the author has succeeded in overcoming by introducing the high-pressure steam into the upper end of the first cylinder, where there is no gland, and where the piston is formed so as to require no lubricating material. The steam is cut off at about half stroke in this cylinder, and when it is admitted for the return stroke into the bottom of the second cylinder, of four times the area, the temperature is so much reduced as to cause no difficulty when brought into contact with the piston-rod gland. From the bottom of the second cylinder the steam expands into the top of the same cylinder, which is of larger capacity than the bottom, and serves as a chamber, and is in direct communication with the valve box of the third cylinder; this last is double-acting, and is arranged to cut off at about a quarter stroke, and at the termination of the stroke exhausts into the condenser, with a total expansion of about thirty-two times. All the cylinders are jacketted with wrought-iron tubes, which are cast in the metal, and supplied with steam direct from the boiler, the condensed water from the jackets being conveyed to the hot well. The whole of the cylinders and valve boxes, &c., are enclosed with a double case of thin sheet iron, filled in with vegetable black to prevent the escape of heat, and at the same time

to maintain all the parts at a high temperature.

In working these high pressures with great expansion the ordinary mode of packing the pistons was found unsatisfactory, and to overcome the difficulty the compound piston was devised. The prevalent scoring and cutting of engine cylinders was effectually remedied by the discovery of the compound metal, of which the packing rings are made, which requires no lubricating material. Many cylinders fitted with piston rings made of this metal have been several years at work, and have been often examined, the cylinders showing no signs of wear, the wear taking place on the rings only, which may be easily and inexpensively renewed as required, and experience has proved that with these pistons, the longer an engine is worked the more perfect does the surface of the cylinders become, and the less wear results to the packing rings. This metal for piston packing rings is composed of five parts tin and fifteen parts copper, and has since been used by several other makers for ordinary engines with great success. When this metal is used, no oil or grease is required to lubricate the cylinders—a great advantage, particularly where the engines are fitted with surface condensers.

The high-pressure pistons in the steamers Atacama and Coquimbo of the Pacific Steam Navigation Company were fitted with these packing rings, and it was reported by the superintendent engineer that the cylinders, which were previously rough and slightly grooved, were in the course of two or three voyages, or about 10,000 miles' run, brought up to a beautiful smooth surface, and had since kept in capital order, giving no trouble whatever. After having been once brought up to a smooth working surface, the packing rings did not wear the cylinders; the wear of the rings was also very slight, and the friction greatly reduced, and one-third of the lubrication necessary for cast-iron rings was found sufficient. In the torpedo vessels made for the French Government, Messrs. Thornycroft found these packing rings for the engine pistons a great advantage, as there was no chance of the cylinders being scored; and they were enabled to run the two hours' trial easily, at the high speed of about 430 revolutions per minute, without using any oil or grease in the cylinders. In an engine at the Dorking Grey Stone Lime Company's works, the manager reported, after 2½ years' use of these packing rings for the piston, that they required no grease of any kind, and worked the cylinders to a polished face and needed no looking to until worn out; a set of rings lasted about 100 days, working at the usual high steam pressure of 400 lbs. per square inch.

The surface condenser used is constructed of a number of vertical tubes in such a manner as to be absolutely tight, so as to insure that the condensing water inside the tubes shall not mix with the water from the condensed steam outside them. The tubes are $\frac{7}{8}$ in. internal and $1\frac{5}{16}$ in. external diameter, welded up at the top end and fixed securely in a tube plate at the bottom. These tubes are fitted with internal tubes, open at both ends, which are fixed in a division plate at the bottom, in order to cause the condensing water to circulate to their extreme ends.

A small still, worked by a steam coil, is used to distil water for replenishing any small waste that may take place in the feed supply. A duplicate apparatus forms part of the ordinary equipment of a sea-going vessel, to furnish steam from sea-water, for blowing the steam whistle, cooking, supplying distilled water for use of passengers and crew, and for all other purposes where distilled water is required.

In designing the machinery described, provision is made for passing any waste steam from the safety valves, &c., into the surface condenser, and the great strength of the boilers allows a margin of 100 lbs. per square inch or more to exist between the load on the safety valves and the pressure required to work the engines. When this system is fully carried out in steamships, the author would deem it quite safe, and more than ample for making good the waste of water from all sources, to provide, beyond the water in the boilers, a supply of fresh water in the proportion of ten gallons per twenty-four hours per 100 indicated horse power. As an instance of the practical feasibility of carrying out the system of machinery that has been described, it may be stated that a

boiler containing only 300 gallons, and an engine working at 250 lbs. pressure and 250 indicated horse power, were worked night and day continuously thirteen days (one Sunday excepted) without requiring any addition to make good the waste of working, nor at the end of the trial was there any appreciable difference in the water level of the boiler.

In the indicator diagrams exhibited, the two upper diagrams are taken from the working of a pair of marine engines on this plan of seventy nominal horse power, and the coal consumed averaged 1.62 lbs. per indicated horse power per hour. In this case there was no vacuum and no low-pressure cylinder, and the terminal pressure was 21 lbs. per square inch above the atmosphere; the boiler pressure was 300 lbs. per inch. With the addition of a low-pressure cylinder and a vacuum, the author considers it may safely be estimated that this system, properly carried out, will realize an average duty of one horse power for each pound of coal per hour.

REVIEW OF THE FOREGOING FROM "ENGINEERING."

Every engineer who has had an opportunity of examining the high-pressure engines and boilers described in Mr. Perkins' paper must, we think, have been struck with the perseverance and ability which the author has brought to bear upon their construction. Steam at pressures of from 300 lbs. to 500 lbs. per square inch is not a fluid to be trifled with, and it is only by the adoption of many novel structural expedients and the employment of the very best workmanship that Mr. Perkins has been able to attain the results he describes in his paper. It may, however, now be taken for granted that Mr. Perkins has proved that engines of moderate size may now be made upon his plans and successfully worked with steam of the pressures named, and the chief points which now appear to us to be open to discussion are: What are the real advantages gained by the use of steam of excessively high pressures? and, second, Can the results attained with engines of moderate power be repeated on a larger scale and under the conditions as to weight, &c., which must be satisfied in the case of marine engines? This latter branch of the question, however, we do not intend to enter into here; and in the present article we shall, in fact, confine ourselves to discussing some of the points included in the first query.

What then are the advantages to be gained by the adoption of steam pressures of, say, 500 lbs. per square inch? The advantages claimed by Mr. Perkins may, we think, be summarized as follows: (1) That by resorting to such pressures we are enabled to use a type of boiler which could not otherwise be employed, this boiler being, it is said, durable, easily manufactured, and safe; and (2) that by using the high-pressure steam, and employing large ratios of expansion, the work can be done with a much less expenditure of fuel than is now required by our best marine engines. Let us consider each of these claims in the order in which we have mentioned them. First, then, as to the boiler. It may, we think, be at once conceded that with steam at moderate pressures it would be quite impossible to successfully work a boiler constructed on Mr. Perkins' system. It is only by employing very high pressures, and thus causing the steam generated to occupy a small volume, that the escape of the steam from the lower pipes to the steam space is rendered possible. But the volume occupied by the steam in the tubes depends not only upon its pressure but also upon the rate at which it is being generated, and it is easy to imagine conditions under which the rate at which the steam is generated may exceed that at which it can escape to the higher tubes, thus involving a risk of failure. It is in fact evident that the durability of the boiler must be largely affected by the rate at which it is worked, and hence the statement that a boiler has lasted so many years loses its value unless we are at the same time informed what work it has been doing. It is much to be regretted that Mr. Perkins' paper fails to give us definite information on these points. An ordinary marine boiler evaporates very commonly 5 lbs. of water per hour per square foot of heating surface, and very often does much more, and what we require to know is whether in the Perkins boiler the rate of evaporation

considered desirable is higher or lower than this, and also how much water can be evaporated per pound of coal when working under what are considered by the designer to be proper conditions. Mr. Perkins will, we feel certain, add much to the value of the facts stated in his paper if he would furnish definite information on these points. To be of value the evaporative results per pound of fuel should be deduced from careful trials lasting at least ten hours, both fuel and water being carefully weighed, and precautions being taken to avoid loss of water by priming. We may add that in comparing Mr. Perkins' boiler with the ordinary marine type it must be borne in mind that the former is, from its small water capacity and other features, lighter than the latter in proportion to its exposed surface, and thus to do a certain amount of work it would be possible for Mr. Perkins to employ, without necessarily incurring excessive weight, a greater area of heating surface than is adopted in ordinary practice.

Next as regards the question of the economy alleged to be attained by the employment of steam at excessively high pressures worked with large measures of expansion; and here we may state at once that, to say the least, we consider this economy to be very generally vastly overrated. The statement made by Mr. Crampton in the course of the discussion on Mr. Perkins' paper, that in case of a certain pumping engine he had obtained with steam of 30 lbs. to 40 lbs. pressure, expanded about five-fold, as good results as with steam at higher pressures used more expansively, appeared to be received by some of the members present with a certain amount of incredulity, yet we believe the statement to be quite consistent with other experiences. To avoid misaprehension it will be advisable that we speak more fully on this point. It must be remembered that in Mr. Crampton's case the load on the engine was constant, and thus the mean effective pressure in the cylinder must also have been constant (neglecting any small differences of friction), whatever the pressure of steam employed might be. With steam at 35 lbs. pressure, or, say, 50 lbs. absolute, expanded five-fold, the total mean pressure would be 26 lbs. almost exactly, while to obtain the same total mean pressure with 70 lbs. steam, would require an expansion of a little over eleven-fold. Mr. Crampton's statement thus amounts to this, that in a single-cylinder condensing engine 35 lbs. steam expanded five-fold more economical—or at least as economical—as 70 lbs. steam expanded eleven-fold, and in making this statement we believe he is practically right. A confirmation of the view here taken is afforded by the experiments of Chief Engineer C. H. Loring and Mr. Charles Emery, on the engines of the U. S. Revenue steamer Gallatin, recorded in our number of February 18, 1876, and by those of Mr. Emery, on the engines of the Bache, of which we gave an account in our issue of January 1, 1875. In the case of the Gallatin trials, the engines were worked with steam at different pressures, and it will be seen on referring to the record of the experiments that when working with about five-fold expansion an increase in the steam pressure from 40 lbs. to 70 lbs. per square inch gave an economy of but about 10 per cent. only, while in the case of the Bache (when the engines were being worked non-compound) an increase in the ratio of expansion from 5.1 to 12.6 (the initial pressure being 80 lbs.) resulted in an increase of steam consumption by about 16 per cent. Coupling these facts we have good evidence in support of Mr. Crampton's statement, and we are warranted in concluding that with the steam of higher pressure the large amount of expansion employed would cause a loss more than counterbalancing the gain in other respects. Had Mr. Crampton been able to increase the load on his engine at the same time that he increased the steam pressure, he would no doubt have got better results at the higher pressures than at the pressures of 30 lbs. to 40 lbs. which he found most beneficial under the circumstances, while had the engine been of the compound type, he would have been able to use larger ratios of expansion with advantage. All recent researches into the action of steam in the steam engine tend to show conclusively that the extent to which steam can be benefically expanded in a single cylinder—even if that cylinder is steam jacketted—is far less than used formerly to be supposed, and that if we are to employ large measures of expan-

sion we must divide the work between two or more cylinders, so as to limit the range of temperature in each. This view was maintained by Mr. Adamson during the discussion on Mr. Perkins' paper, and there can be little doubt of its correctness.

But while we have evidence that by the employment of the compound system we may benefically use measures of expansion not otherwise desirable, we are as yet without experimental data to show us the limit to which we can advantageously go under such conditions as Mr. Perkins proposes to introduce. All we know is that the economy actually obtained by the adoption of high pressures and correspondingly large measures of expansion increases far less rapidly than theory might at first lead us to expect, and reasoning from analogy we see good reason to anticipate that at pressures lower than those advocated by Mr Perkins the advantages derivable from the adoption of a large increase of expansion *per se* are more than counterbalanced by the losses due to increased radiation and variations of temperature in the cylinders, to say nothing of leakage past pistons and kindred matters. Under these circumstances it is we think very desirable that statements as to the economy derivable from the employment of steam of very high pressures should be founded on a sound basis. So far as we have been able to gather from Mr. Perkins' paper, and the discussion which succeeded it, no experiments have yet been made which justify this economy being stated as an absolute fact. Mr. Perkins appears to have calculated the consumption of steam per horse power per hour from indicator diagrams, but such a mode of calculation is entirely untrustworthy, as all who have conducted many engine trials well know. Considering that several of Mr. Perkins' engines are now at work, it ought not to be difficult to arrange an exhaustive trial which would afford data at present entirely wanting, and which it is necessary should be available before the merits of Mr. Perkins' system can be correctly judged. In conclusion, we may say that in making these remarks we in no way desire to criticise Mr. Perkins' system adversely. The skill with which Mr. Perkins has developed the use of high pressure steam deserves every recognition at our hands; but at the same time we believe that we are serving the best interests of Mr. Perkins, as well as of the public, by directing attention to some points connected with that system which are at present doubtful, and which should be cleared up without unnecessary delay.

RELATION AMONG THE ULTIMATE RESISTANCES OF MATERIAL TO TENSION, TO COMPRESSION, AND TO CROSS-BREAKING.

By JOHN D. CREHORE.

Written for VAN NOSTRAND'S MAGAZINE.

So long ago as 1842 the indefatigable Hodgkinson communicated to the British Association for the Advancement of Science, as one result of his experiments, the discovery of a "direct relation between the conditions of rupture by transverse and by longitudinal strain." The first three columns of the following table, contain a summary of his results as given by Mr. Hodgkinson himself, and may be found in many textbooks on engineering:

Material.	1	2	3	4	5
Timber	1000	1900	85.1	1531.8	1343.5
Cast-Iron	1000	158	19.8	356.4	323.6
Stone	1000	100	9.8	176.4	239.9
Glass	1000	123	10	180	269.7

Column 1 contains assumed crushing strength per square inch.

Column 2, mean relative tensile strength per square inch.

Column 3, mean relative transverse strength of a bar one inch square and one foot long, loaded at the center.

If we multiply each number in column 3 of this table, by 18, we derive the corresponding number in column 4, which is thus made to represent the relation of the "modulus of rupture" to the ultimate resistances to compression and tension, whose ratio is given in the 1st and 2nd columns.

To explain this change, let

W = breaking weight suspended at center of bar.

B = the modulus of rupture required.

Then, since each bar is one inch square and one foot in length between supports, we have the moment due the external forces,

$$M = \tfrac{1}{4} W \times 12 = 3\,W;$$

and the moment due the internal forces,

$$R = \tfrac{1}{6} B = M = 3\,W.$$

Whence $B = 18\,W.$

Column 5 contains relative values of the moment of rupture as computed by formula (5) below; from the assumed numbers in the 1st and 2nd columns of the same table.

It should be remembered that no one of the numbers in this table, necessarily represents the actual ultimate resistance of the given material, to tension, compression, or cross-breaking. They are abstract numbers, merely denoting ratios which the experimenter deduced from such trials as he had made. Other laborers have since wrought in the same field; and it is the object of this paper to give a formula which shall express the relation we are seeking, in accordance with a wider range of experimental facts than have been determined by any one observer; and by this means to clear up certain doubtful points, and remove some confusion which mars the pages of a few of our best instructors.

For instance, in the well known and most admirable work of Mr. Bindon B. Stoney on the Theory of Strains, we find at the 84th page, article 131, these statements:

"The student will naturally conclude that the formulæ investigated in the present and preceding chapters should give identical, or nearly identical results, when they are applied to the same girder; that, for instance, the breaking weight of a solid rectangular semi-girder, calculated by the equation $Wl = adS$, should closely agree with its breaking weight calculated by the equation $6Wl = fbd^2$; and, if our theory were complete, this would no doubt be the case. To test its accuracy, let us compare these two equations, when we obtain this result,

$$S = \tfrac{1}{6} f,$$

that is, the value of S for solid rectangular girders of any given material should equal one-sixth of the ultimate tearing or crushing strength of that material, according as it yields by tearing or crushing. In many instances, however, this will be found to be far from the truth; for example, the value of S for small rectangular bars of cast-iron = 3.4 tons, and 6 times this = 20.4 tons, far exceeds the tensile strength of ordinary cast-iron, which is about seven or eight tons per square inch. It must, indeed, be confessed that the law of elasticity ceases to be applicable when we approach the limits of rupture; and that the formulæ for *solid* girders investigated in the present chapter give their breaking weight much under what it really is for many materials, and this discrepancy will probably be found more marked in those whose ultimate tearing strain differs widely from their ultimate crushing strain. Greater confidence, however, may be placed in the formulæ relating to hollow and flanged girders.

Mr. Hodgkinson endeavors to explain this discrepancy by a change in the position of the neutral axis, as soon as the limit of elastic reaction of the horizontal fibres has been passed, and gives some reasons for this hypothesis derived from experiments on cast-iron, in his *Experimental researches on the strength of cast-iron*, p. 384. This seems a plausible hypothesis, for if the neutral axis of a solid rectangular cast-iron girder approach its compressed edge as the weight increases, and, after the limit of tensile elasticity has been passed by the fibers along the extended edge, we shall have a larger

proportion than one half the girder subject to tension, and consequently the total horizontal tensile strain may exceed that derived from our theory, which assumes that the neutral axis always passes through the center of gravity of the cross-section.

These views of Hodgkinson are corroborated by experiments made by Duhamel, and also by similar experiments of the elder Barlow; but Mr. W. H. Barlow controverts these views, and gives the results of micrometrical measurements, on two cast-iron rectangular girders which he subjected to transverse strain, and infers from these experiments "that the neutral axis does not shift its position, and this view seems in accordance with experiments made long ago by Sir David Brewster," on polarized light transmitted through a rectangular glass girder.

I shall make no attempt to settle this dispute by preponderance of authority, but shall simply enunciate the law which seems to me axiomatic and inevitable *a priori*, and then apply it to the interpretation of the results of actual experiment, leaving the truthfulness of the law to be inferred from its accordance with observed facts.

I shall here consider only the case of a solid rectangular beam, and let us take

h = depth of beam,

b = breadth of beam,

l = length of beam,

x = distance of the neutral surface from the *compressed* side of the beam.

C = ultimate resistance of the material to crushing by direct thrust, in pounds per square inch.

T = ultimate resistance of the material to extension, in pounds per square inch.

B = the unit strain which, at the instant of rupture, would be developed at the upper and lower surfaces of a beam having its neutral surface midway between these outer surfaces; that is, B = the modulus of rupture, also in pounds per square inch.

Then we have, for the compressed part of any cross-section, moment of internal forces.

$$M = \tfrac{1}{6} C b h^2;$$

and for the extended part of the same cross-section,

$$M = \tfrac{1}{6} T b (h-x)^2.$$

Now since, in the nature of things, or upon the principle of sufficient reason, these two moments must be equal to each other, and each also equal to $\frac{1}{6} B b (\frac{1}{2}h)^2$, we have

$$C x^2 = T(h-x)^2 = B(\tfrac{1}{2}h)^2 \quad . \quad . \quad (1)$$

Whence,

$$x = h \frac{\sqrt{T}}{\sqrt{C}+\sqrt{T}} = \tfrac{1}{2} h \sqrt{\frac{B}{C}} \quad . \quad . \quad (2)$$

$$C = \frac{B}{\left(2-\sqrt{\frac{B}{T}}\right)^2} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$T = \frac{B}{\left(2-\sqrt{\frac{B}{C}}\right)^2} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$B = \frac{4C}{\left(1+\sqrt{\frac{C}{T}}\right)^2} = \frac{4T}{\left(1+\sqrt{\frac{T}{C}}\right)^2} \quad (5)$$

When, therefore, any two of the quantities C, T, and B, are given, the third may be found, and also the position of the neutral surface.

The following table shows the result of applying these formulæ to cast iron, wrought iron, steel, wood, and stone, using such experimental values for two of the three quantities, as are to be found in the works of Rankine, Stoney, Moseley, Mahan, Weisbach, Vose, Ure, Haswell, &c.

(*See Tables on following pages.*)

The value which has no initial annexed, is the one that has been computed by formula. The authorities are thus abbreviated:

H. Hodgkinson,	R. Rankine,
S. Stoney,	N. Nelson,
Ha. Haswell,	M. Moore,
F. Fairbairn,	K. Kirkaldy,
Bv. Bevan,	Ro. Rondelet,
Bu. Buchanan,	Mo. Moseley,
B. Barlow,	Re. Rennie,
D. Denison,	C. Clark.
W. Weisbach,	

In the case of cast-iron, an empirical coefficient $(1+m)$ whose mean value seems to be about $\frac{1}{9}$, is required to make the value of B as determined by formula, accord with the results of experiment. This is equivalent to giving the same coefficient to C and T, and is supposed to be required by reason of the greater resisting powers of a cast-iron bar at its surfaces than in its central parts.

It will be observed that Stirling's Nos. 2 and 3, Devon No. 3, (hot blast), and Coed Talon No. 2 (cold blast), irons of high tensile strength, require no increment, but give B a value fully up to the extreme experimental number.

The first 9 cast-irons of this table yield Hodgkinson's proportionals almost exactly, with $\frac{1}{9}$ for the coefficient of B.

The names of the 16 irons whose mean strengths are given in the 13th line of the table, may be found in Mr. Stoney's volume, pages 228 and 289. But as I have nowhere found the moduli of rupture due those 16 irons, I insert Mr. Fairbairn's extremes beside the augmented value of B.

When I had advanced, in the application of these formulæ, through cast-iron, wrought-iron, steel, and wood, there fell into my hands Professor Thurston's article on the Flexure of Beams, appended to Du Bois's translation of Weyrauch; and here I learned that the law which I had called "axiomatic and inevitable *a priori*," is not now first discovered, but is "an early hypothesis of Navier, which seems to have been entirely abandoned by him subsequently, and which has not been accepted by subsequent writers on the subject." This last remark may partially excuse my ignorance of the existence of such an hypothesis; and the following words of Professor Thurston, together with the results of experiment which he brings to my aid, may mitigate the rashness of my endeavor to reinstate the rejected hypothesis:

	Cast-Iron.	Ultimate Resistance to Crushing. C	Ultimate Resistance to Extension. T	Position of Neutral Surface. $\frac{x}{h}$	Ultimate Resistance to Cross-breaking. B	Increment given to B. m	$(1+m)$ B	As given by Authorities, B
1	Carron No. 2 (cold blast).	106,375 H.	16,683 H.	.28	34,240	$\frac{1}{8}$	38,520	38,556 H.
2	Carron No. 2 (hot blast) .	108,540 H.	13,505 H.	.26	29,520	$\frac{1}{4}$	36,900	37,503 H.
3	Carron No. 3 (cold blast).	115,442 H.	14,200 H.	.26	31,132	$\frac{1}{7}$	35,579	35,980 F.
4	Carron No. 3 (hot blast)..	133,440 H.	17,755 H.	.27	38,129	$\frac{1}{9}$	42,365	42,120 F.
5	Devon No. 3 (hot blast)..	145,435 H.	21,907 H.	.28	45,476	0	45,476	45,278
6	Buffery No. 1 (cold blast)	93,366 H.	17,466 H.	.30	34,044	$\frac{1}{10}$	37,448	37,503 H.
7	Buffery No. 1 (hot blast).	86,397 H	13,434 H.	.28	27,640	$\frac{2}{9}$	35,540	35,316 H.
8	Coed Talon (cold blast)..	81,770 H.	18,855 H.	.32	34,461	0	34,461	33,858 F.
9	Coed Talon (hot blast)...	82,739 H.	16,676 H.	.31	31,771	$\frac{1}{10}$	33,359	33,145 F.
10	Means of these 9.....	105,945	16,720	.29	34,046	$\frac{1}{9}$	37,829	37,695
11	Hodgkinson's Ratios.....	1,000 H.	158 H.	.29	321	$\frac{1}{9}$	356.7	356.4 H.
12	W. H. Barlow.....	110,022	18,750 B.	.29	37,575	$\frac{1}{9}$	41,750 B.	41,750 B.
13	Means of 16 Irons.....	86,284 H.	15,298 H.	.30	30,245	$\frac{1}{9}$	33,606	27,297 to 48,195 F.
14	Stirling's No. 3.....	144,264 H.	23,461 H.	.29	47,658	0	47,658	27,297 to 48,195 F.
15	Stirling's No. 2.....	122,395 H.	25,764 H.	.31	48,426	0	48,426	27,297 to 48,195 F.
16	Weisbach.....	146,000 W.	19,592 W.	.27	41,980	$\frac{1}{9}$	46,644	30,000 to 46,900 W.
17	Rankine, means.....	112,000 R.	16,500 R.	.28	34,466	$\frac{1}{9}$	38,295	38,250 R.
18	Stoney, means.....	87,490 S.	16,295 S.	.30	31,806	$\frac{1}{9}$	35,340	30,240 to 45,696 S.

	Material.	Compression.	Tension.	Position of Neutral Surface.	By Formula.	By Authority.
	Wrought Iron.	C	T	$\frac{x}{h}$	B	B
19	Plates	36,000 R.	51,000 R.	.54	42,524	42,000 R.
20	Bars	40,000 R.	68,848 K.	.57	51,522	51,341 C.
21	Stoney, means	40,320 S.	57,555 K.	.55	47,793	30,240 to 74,995 C.
22	Rankine, means	38,000 R.	65,000 R.	.57	48,815	42,000 to 54,000 R.
	STEEL.					
23	Hammered Bessemer	225,568 F.	83,391 F.	.38	128,998	128,083 K.
24	Rolled Bessemer	214,843	71,658 K.	.37	....	115,181 K.
25	Hammered Crucible	314,714	85,546 K.	.34	....	147,840 K.
26	Rolled Crucible	250,702	68,589 K.	.34	....	118,272 K.
	WOOD.					
27	Ash	9,363 S.	16,700 S.	.57	12,246	12,156 S.
28	Ash	9,000 R.	17,000 R.	.58	12,062	12,000 to 14,000 R.
29	Beech	9,363 S.	11,500 S.	.53	10,348	8,280 to 10,434 S.
30	Beech	9,360 R.	11 500 R.	.53	10,348	9,000 to 12,000 R.
31	Birch	9,032 S.	15,000 S.	.56	11,454	8,010 to 12,366 S.
32	Box	10,300 R.	20,000 R	.58	13,965	14,670 S.
33	Cedar of Lebanon	5,860 R.	11,400 R.	.58	7,948	7,400 R.
34	Deal	7,293 S.	12,900 S.	.57	9,505	9,372 S.
35	Deal, means	6,939 S.	12,900 S.	.58	9,237	9,372 S.
36	Elm	10,331 H.	14,400 Bv.	.54	12,113	11,820 DN.
37	Elm	10,331 H.	5,780 B.	.43	7,569	7,850 R.
38	Fir (Spruce)	6,499 H.	12,000 B.	.58	8,627	8,076 M.
39	Fir (Red Pine)	5,787 R.	13,000 R.	.61	8,328	8,320 R.
40	Fir (Larch)	5,570 R.	9,500 R.	.57	7,146	5,000 to 10,000 R.
41	Fir (Larch)	5,568 H.	10,220 Ro.	.58	7,372	8,010 B.
42	Fir (Larch)	3,201 H.	8,900 Bv.	.62	5,003	5,466 D.
43	Mahogany	8,200 R.	14,900 R.	.58	10,811	10,314 to 11,500 R.
44	Oak, means	7,923 S.	15,425 S.	.58	10,754	10,400 S.
45	Oak, means	7,900 R.	13,350 R.	.57	10,105	10,400 R.
46	Oak (average)	7,912	14,388	.57	10,429	10,400
47	Pine, means	6,584 S.	10,634 S.	.56	8,248	8,457 S.
48	Sycamore	7,820 H.	13,000 Bv.	.55	9,377	9,600 R.
49	Teak, Indian	12,000 R.	15,000 R.	.53	13,375	12,000 to 19,000 R.
	STONE.					
50	Granite, means	8,839 S.	586	.21	1,483 S.	1,584 Ha.
51	Sandstone	7,189 Bu.	1,158 Bu.	.29	2,359	1,100 to 2,360 R.
52	Marble, white	6,318 Re.	551 H.	.23	1,314	1,252 H.
53	Marble, black	14,833 S.	1,072	.21	2,664 Mo.	2,697 H.
54	Limestone	9,753 S.	857 W.	.23	2,039	2,007 S.

"The assumption that the resistances vary each way from the neutral surface proportionally with their distance from that surface, is, when coupled with a rejected hypothesis of Navier, nevertheless, not far from the truth in special cases, as

may be shown by proper mathematical treatment and comparison with results obtained experimentally."

He then gives the results of experiments made by Mr. William Kent, upon cast and wrought iron and ash timber, at the Stevens Institute of Technology.

His results, reduced to conform to my notation, are as follows:

Material.	C	T	$\frac{x}{h}$	B	m	$(1+m)$ B	BEx.
Cast-Iron.....	96,000	16,000	.29	32,280	$\frac{1}{2}$	34,970	35,000
Wrought-iron.....	60,000	60,000	.50	60,000	..	....	60,000
Ash.....	9,000	17,200	.58	12,120	..	....	12,000

The numbers harmonize sufficiently with those I had been able to gather from the various authors I have named; and, moreover, I suppose Mr. Kent's experiments were made by the same hand, and all upon material of the same quality. Such deductions, manifestly, are worthy of more confidence than should be given to ratios deduced from the tests of different observers, applied to different qualities of material; unless, indeed, the number of observations be very great, in which case the mean values of the observed ratios should agree closely with the requirements of the formula.

Whether that perfect and all-comprehensive formula which Prof. Thurston so finely pictures to the mind, shall ever be written, or not, it is evident that engineers need not be led very far from the truth in this matter, if they will correctly interpret the teachings already accumulated (and accumulating) by experiment.

When Mr. Stoney finds conflict between experiment and his equation $6S=f$, he should examine and see if f, *can*, as he has used it, represent anything but B, as I have defined it above. Giving f this value, his equation involves no wide departure from experimental data. Thus, if S=3.4 tons, then f=20.4 tons=45,696 lbs., which, though high, is quite within the limits of observation.

In regard to the permanence of the neutral surface in one position, I offer these queries, viz.

If the neutral surface *does* change its position, does it not also change its length? And if it changes its length, could Mr. W. H. Barlow's "micrometrical measurements" have detected its final position, or any change in its position?

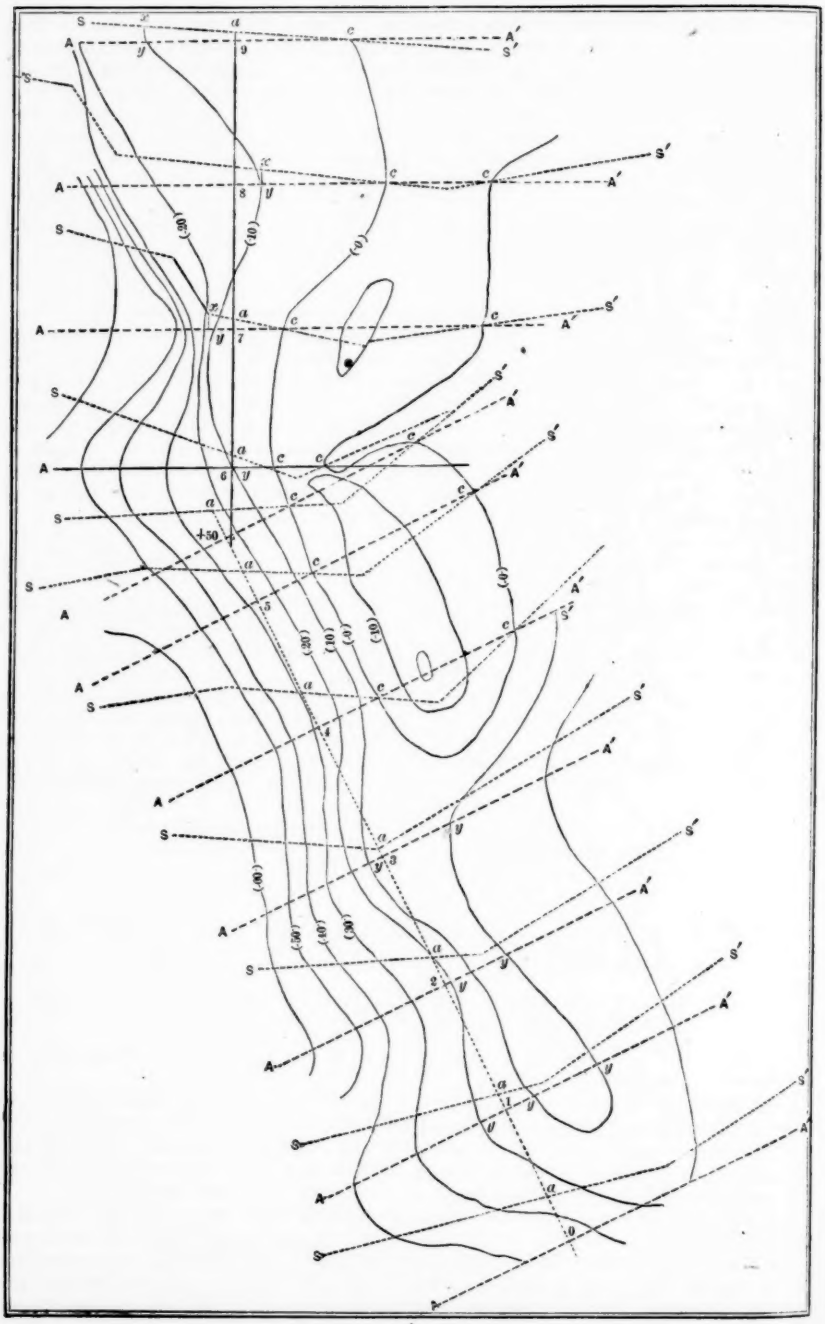
Is there any absurdity involved in supposing the neutral surface to change its position with every increment of strain in rectangular beams whose material yields unequally to tension and to compression? Or, rather, has not each value of the strain a neutral surface peculiar to its magnitude, regardless of the previous or subsequent values of the strain?

CONTOUR LINES IN RAILWAY SURVEYS.

By A. S. HARDY, Professor of Civil Engineering in Chandler Scientific School.

Written for VAN NOSTRAND'S MAGAZINE.

ACCURACY in the topographical features of the map may often be dispensed with, and on the other hand is often quite important. The art of sketching in these features by the eye may be carried with practice to a high degree of accuracy, so far as the general disposition of the surface undulations is concerned. But where the true position of contour lines is desired, *that is, the true relation between horizontal and vertical distances*, no sketch of this character is reliable. The writer has found this especially true with railroad parties in the course of practical instruction, where the topographer could not be depended upon for even general features, and where time was wanting for the delineation of contour lines by well known accurate methods. The following combi-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(0)
+50

nation of clinometer and profile notes resulted from an effort to obtain from the preliminary survey a map reliable for location in the office. The principles are well known and their combination in the form below may have been before employed, although never having come to the writer's notice. Having plotted the preliminary line (see figure,) draw through each station as 0, 1, 2, 3, &c., lines AA′, AA′ &c. perpendicular to the axis, and lay off the distances 0*a*, 1*a*, 2*a*, &c., along the axis and equal to the "height of surface" at the station in question above the datum plane, taken from the profile look, and at the points *a*, *a*, &c., thus determined, plot the cross sections SS′, SS′, SS′, &c., from the clinometer notes. These are taken in the field with a light rod and tape in the usual manner. Then are AA′, &c., traces *on the datum plane* of the vertical planes of cross-section, and SS′, &c., the revolved positions of the intersections of these planes with the surface. If then within the limits of the survey these perpendiculars and slope lines intersect, as at *c*, *c*, *c*, &c., the line *ccc*, &c., will be the contour line cut by the datum plane. If contours are to be drawn 10 feet apart take a distance $xy=10$ feet (stations 8 and 9) with the dividers between the slope SS′ and the line AA′ at every station. The points *x*, *x*, &c., are the revolved positions of points on the contour line 10 feet above *ccc*, &c., and in true position in plane are at *y*, *y*, &c., and *yyy*, &c., is the contour 10 feet above the datum. Take next a distance 20 feet and repeat. Contour lines may be thus sketched in very rapidly and with remarkable accuracy. The lines AA′ &c., and SS′ &c., may be drawn very lightly. As the same scale is used throughout, should any of the points *a*, *a*, &c., be over 100 feet above the datum plane, to avoid confusion when the stations are 100 feet apart assume any new datum plane above the former one. If, for example, the plane be raised 50 feet, the contour line which, referred to the former plane was (50), will be continued as the (0) contour for the new plane. Whenever the transverse slopes are uniform, the contours will be equidistant at that station—contours below the datum plane, should any occur, are located of course in a similar manner.

In the location of a few miles of road on the steep slopes of a river valley, where the exact topography was indispensable, the writer was surprised at the rapidity with which the contours were delineated, as well as by the accuracy of the location as subsequently proved in the field. Although more especially adapted to the topography of a narrow strip, as in a railroad survey, its extension to ordinary mapping is obvious.

INVESTIGATION OF THE QUESTION OF THE THRUST OF EARTH BEHIND A RETAINING WALL.

By J. ROMILLY ALLEN, A.I.C.E.

Written for Van Nostrand's Magazine.

PROBLEM TO FIND THE HORIZONTAL EARTH THRUST AGAINST THE BACK OF A RETAINING WALL.

It is assumed that the earth, being confined by the wall, acts as one solid mass and that if the wall were removed the earth would break away along a plane surface called the "Plane of Rupture."

On this supposition, therefore, the thrust behind the wall is produced by a wedge of earth, included between the back of the wall and the Plane of Rupture.

It must be premised that the position of the Plane of Rupture (which depends on the angle of repose of the earth and the batter of the back of the wall) has yet to be determined.

The Plane of Rupture does *not* coincide with the natural slope of the earth, as might be supposed at first sight; for the prism of earth, included between the back of the wall and the natural slope,

as long as it is kept together in one solid mass, is just supported by the friction along the plane of natural slope, and consequently in this case no thrust will be produced.

For a similar reason, it is evident that the Plane of Rupture does not lie between the natural slope and the horizontal, and therefore it must fall in some position intermediate between the natural slope and the back of the wall.

Now if all prisms of earth produced the same amount of thrust against the back of the wall, the position of the plane of rupture would be indeterminate, since the earth would be just as likely to break away in one direction as another.

There is, however, one prism which produces greater thrust than any other, and this is obviously the one which will break away first.

Consequently the plane along which this prism slides is the plane of rupture.

It is, therefore, necessary to find the "Prism of maximum Earth Thrust," which is done as follows:

Note.—Friction against the back of the wall is neglected.

The portion of wall and bank dealt with is supposed to be one foot in length, so that this factor is left out in the calculations.

Fig. 1 shows the cross section of wall and bank of earth supported.

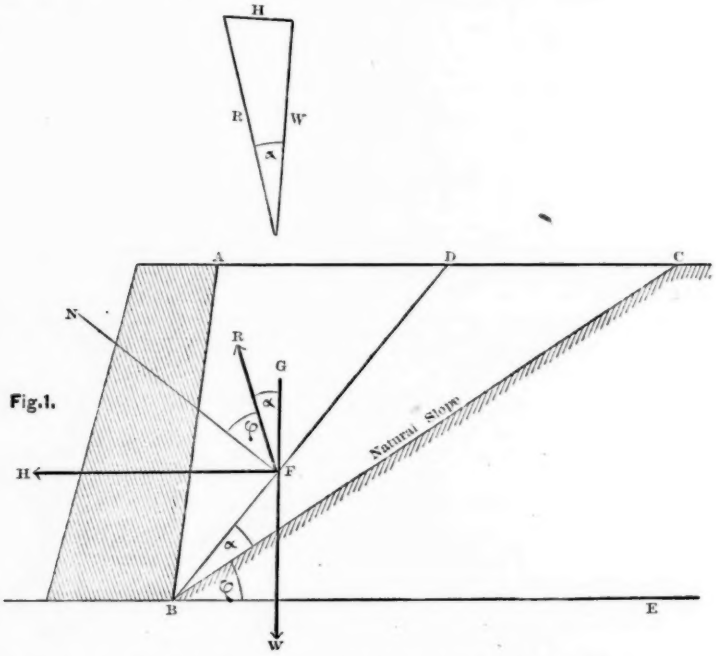


Fig. 1.

AB is the back of the wall.

BC the natural slope of earth.

BE the horizontal.

Consider the condition of any prism of earth such as ABD, resting on the plane BD, which falls between the natural slope and the back of the wall.

The three forces which keep ABD in equilibrium are:

1. The weight W of ABD
2. The horizontal thrust H against AB
3. The reaction R of the plane BD

Let the angle of repose CBE, of earth $=\phi$ and angle DBC which DB makes with natural slope $=\alpha$.

Now the directions of the three forces W, H, and R, are as follows:

1. W acts vertically through G, the center of gravity of ABD.

2. H acts horizontally.

3. R acts at an angle=ϕ with FN the normal to BD.

Draw the Triangle of Forces, having its sides respectively parallel to these three directions, and make the side W of such a length as to represent the weight of prism ABD, to any fixed scale.

This construction gives the value of H to the same scale.

Now it may easily be shown, by producing the normal NF to cut BE, that angle RFG=α, and, therefore, that the corresponding angle of the triangle of forces between R and W also=α.

Therefore

$$H = W \tan. \alpha \quad . \quad . \quad . \quad . \quad (I)$$

But W, which represents the weight of a prism of earth ABD, one foot in length, is proportional to area ABD.

So that H attains its maximum value when area ABD$\times$tan. α is greatest.

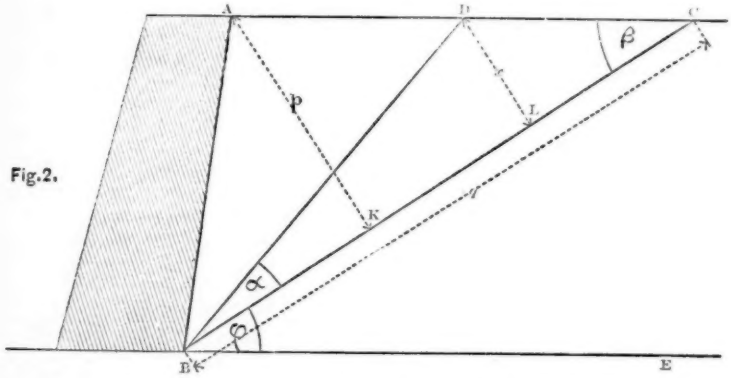


Fig.2.

In order to investigate this, in Fig. 2 let fall perpendiculars AK, DL, on natural slope BC.

Suppose Constant Quantities

p=AK, length of perpendicular from top of wall on natural slope.

q=BC, length of natural slope.

β=angle ACB, between surface of ground and natural slope.

Variable Quantities;

x=DL, length of perpendicular from D on natural slope.

α=angle DBC, which DB makes with natural slope.

Now

$$\text{Area ABD} \times \tan. \alpha = (\tfrac{1}{2}pq - \tfrac{1}{2}xq) \tan. \alpha$$

$$= \tfrac{1}{2}q\,(p-x)\left(\frac{x}{q - x \cot. \beta}\right)$$

$$= \tfrac{1}{2}q \cdot \frac{px - x^2}{q - x \cot. \beta}$$

Differentiating the quantity $\dfrac{px - x^2}{q - x \cot. \beta}$ and putting the value thus obtained=0 for a maximum, there results the following equation

$$(q - x \cot. \beta)\,(p - 2x) - (px - x^2)(-\cot. \beta) = 0$$

or

$$x^2 \cot. \beta - 2qx = -pq \quad . \quad . \quad (II)$$

Putting this equation into another form

$$pq - qx = qx - x^2 \cot. \beta$$

$$= x(q - x \cot. \beta)$$

$$= x \times \text{BL}$$

$$\text{Area ABC} - \text{Area DBC} = \text{Area DBL},$$

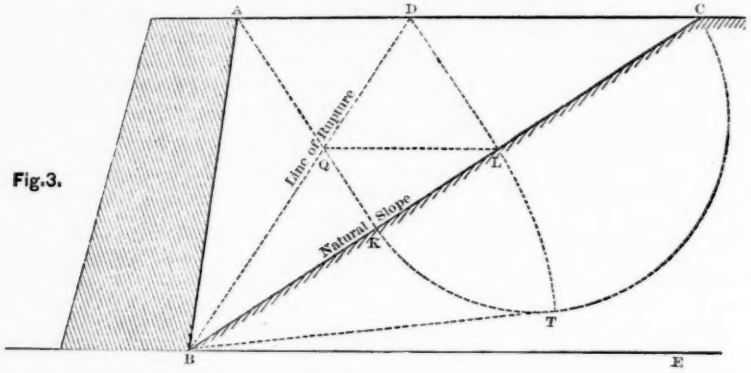
$$\text{Area ABD} = \text{Area DBL} \quad . \quad (III)$$

which equality expresses the only condition necessary, in order that ABD may be the "Prism of Maximum Earth Thrust" and BD the Plane of Rupture.

The following geometrical construction gives the position of BD, subject to the above condition.

Construction—From A (Fig. 3) let fall perpendicular AK on BC, and on KC describe a semicircle KTC.

Fig.3.



From B draw the tangent BT to semicircle.

In BC make BL=tangent BT.

Draw LD at right angles to BC, cutting AC in D.

Join BD, cutting AK in Q.

Now BD is the Line of Rupture:

Proof—Since

$$BL^2=BT^2$$
$$=BC.BK$$
$$\frac{BL}{BC}=\frac{BK}{BL}$$
$$=\frac{BQ}{BD}$$

Join QL

$\therefore$ QL is parallel to DC

And since AK is parallel to DL

$\therefore$ AQ=DL

$\frac{1}{2}$ AQ.BL=$\frac{1}{2}$ DL.BL

$\therefore$ Area ABD=Area DBL

which was the condition insisted on.

The actual amount of maximum Earth Thrust is found thus:

From eq. (I)

H=(weight of prism ABD) $\times$ tan. α
=(weight of prism DBL) $\times$ tan. α
$=w\times\frac{1}{2}$ DL.BL$\times\frac{DL}{BL}$

$$H=\tfrac{1}{2}wx^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (IV)$$

where

H=maximum Horizontal Earth Thrust per foot forward

w=weight of 1 cubic foot of earth

x=DL

x may either be found by solving quadratic eq. (II) or may be thus expressed in terms of known quantities.

x=DL
=LC tan. β
=(BC$-$BL) tan. β
$x=(BC-\sqrt{BC.BK})$ tan. β . (V)

The above formulæ apply equally well whether the surface of the earth behind the wall be horizontal or not when the back of the wall is vertical of height=h and the surface of the bank horizontal

$$H=\tfrac{1}{2}wh^2\frac{1-\sin.\ \alpha}{1+\sin.\ \alpha} \quad . \quad (VI).$$

THE MECHANICAL THEORY OF HEAT.*

From "The Engineer."

THE subject of this work is one which has become increasingly popular in proportion as it has been studied by the application of the sciences of mechanics and numbers. Like several other branches of science, it has become more interesting, and of wider importance, as the cobwebs of unsupported speculation have

* "Treatise on the Mechanical Theory of Heat and its Applications to the Steam Engine, &c." By R. S. McCulloch, C.E., Professor of Mechanics in Washington and Lee University, Lexington, Va. New York: D. Van Nostrand; London: Trubner & Co. 8vo.

been removed under the light of experimentally ascertained facts. It is true that much of what we know of the nature of heat, as far as its analogy to light is concerned, was dimly foreseen by some ancient writers; but, as the author of this work observes, imaginations are of no value, and of little merit, so long as they remain barren of positive results. The first chapter of this work is a brief historical sketch, which places before the reader the names and the nature of the work and claims of those to whom we are chiefly indebted for the development of our knowledge on this subject as we now understand it. Commencing with a reference to the celebrated "Traité de la Lumière," in which Huygens, in 1690, published his demonstrations of reflexion and refraction, regarded as phenomena of wave propagation, and commenting on the Newtonian hypothesis that light and heat are matter, one which obtructed the progress of knowledge for more than a century, the author passes on to the work of Young and Fresnel, laying much stress on the claims of the latter. The remainder of the chapter, and the largest part of it, is chiefly occupied by the result of the work of those who originated and developed the mechanical theory of heat—Rumford, Seguin, Mayer, Colding, and Joule, of course, occupying most attention. A spirit of fairness characterizes this chapter, in which, while giving full credit to Rumford, Mayer, and others, Joule is accredited with the merit of having caused the true theory of heat to meet with general reception. Perhaps a little too much is claimed for Huygens and Fresnel, for while admitting the splendid achievements of both, it must be remembered that until Rumford's experiments upon the mechanical equivalence of heat, his practical and indisputable proof by measurement, their work and discoveries remained so far in the condition of hypotheses, that they could be and were received by some and rejected by other equally eminent men. The proof of actual measurement by mechanical means, was necessary to remove the possibility of the existence of difference of opinion in equally competent minds, and for this proof we are most indebted to Rumford and Joule. The experiments of Hirn are not forgotten in this chapter, full reference being given to his determination of the quantity of work done by heat, the inverse of the problem solved by Joule, and which he determined by a series of experiments with 100-horse power engines. By these experiments it was proved that the efficiency of the Woolf engine was as much as from one-eighth to one-seventh, or about double that obtainable according to the calculations of Regnault, which led him to believe the steam engine a more wasteful machine than it really is. The apparent difference between Regnault's conclusions and actual measurement was removed by Rankine, who explained it in 1849 by showing that a certain quantity of work was done by the steam which had given up heat by condensation in the cylinder. For the purpose of demonstrating this Hirn had glass plates let into a pipe attached to engines working with a pressure of 75 lbs. per square inch, by which he was enabled to see the steam, which was transparent at 307 deg., become clouded with liquid vesicles during expansion.

It should be here said that Mr. McCulloch's book is an advanced scientific treatise designed principally for the use of students, and without any pretence to being a popular treatise. To be able to appreciate the mass of information, the unbroken chain in which the many different parts of the subject form appropriately arranged links, and the deduction from these, the reader must possess at least an elementary knowledge of the calculus and of analytical geometry. To enable the student to make intelligent progress in the study of the mechanical theory and equivalence of heat, it is, of course, necessary that he should well understand the fundamental laws of mechanics, and for this purpose the second and third chapters of the work before us deal with dynamics, in order that a proper conception may be formed of the ideas conveyed by the terms, "work—energy—potential and kinetic—force—conservation and dissipation of energy—transformation of energy, &c. &c., particularly in their thermal applications." Little need be said of these two chapters. As brief expositions of abstract dynamics they are lucid and careful, but in some instances they are too brief to be of any service to the

student, and this is particularly noticeable with reference to the "work of heat" and "work of expansion," which are about sufficient explanation, and are dealt with in about one and a-half page. Chapter IV. is devoted to a discussion of the general laws of heat and thermodynamics, and Chapter V. deals with "airs and vapors," and presents able examinations of the experiments and laws of Gay Lussac, Marriotte, and Rankine, of the influence of temperature upon compressibility, expansion of vapors. In the three succeeding chapters, the most recent interpretations of the known phenomena of heat are examined and applied under the heads of "Internal Energy," "Air Engines," and "Thermal Laws" respectively. Part II. of the work deals with "applications," the first and second chapters of this part. Nos. IX. and X. form a carefully-considered digest of most of the laws relating to steam and vapors as bearing upon thermodynamic questions. "Steam engines, their defects and improvements" are the subjects of the XIth chapter, and Chapter XII. consists of miscellaneous applications. Want of space prevents our reference at any length to the former of these two chapters, or at all to the last. There are some expressions, however, which show that the author has not sufficiently considered his eleventh chapter to make it of service to either engineer or student. For instance, under the head of "Defective Expansion," the author says that "incomplete expansion is one of the principal defects of steam engines," and "the table of Clausius shows that between the temperatures of 150 deg. and 50 deg. C. the expansion of saturated steam is 25.7, or nearly 26 times its original volume. For such dilatation cylinders of enormous size would be requisite."

The latter sentence is practically the only one which suggests that there would be any difficulty in what the author terms complete expansion, and it is disappointing in such a work to find no mention of the principal difficulty to such ranges of expansion—namely, that very reduction of temperature which he has mentioned, or the refrigeration due to expansion, a question which could not be left out of sight with propriety, unless some perfectly non-conducting material had been found in which to conduct such expansion without positive loss. Again when considering the use of superheated steam, the author, in common with many other writers on the subject, observes, "the *chute de chaleur* or difference ($\tau\ \tau^0$) upon which the duty of an engine depends cannot, therefore, be much increased by making the boiler hotter," *i.e.*, because a pressure of ten atmospheres with a corresponding temperature of 180 deg. C. "cannot be much exceeded without danger of explosion." It would, no doubt, occur to most engineers that if the duty of an engine depended alone on the range of temperature in the cylinder, that the difficulty of making sufficiently strong boilers would soon be overcome. With a perfect ideal engine the *chute de chaleur* would undoubtedly be a measure of its efficiency, but in considering the applications of thermodynamics to practical engines, the reader might have expected that some inquiry would have been made into the economical limit to this range of temperature and the conditions which determine that limit. It may be said that this question is somewhat apart from the object of this work; but as practical applications are considered, it seems to us that this important practical question should have been discussed.

In this work Watt is of necessity very frequently referred to, and full justice is done to the magnificent results of his invention and work, but why the author should have designated him Sir James Watt is unexplained. This title was offered to Watt a few years before his death, but, although pleased with the kindness, he declined to accept it, a decision which if he had lived now, he would perhaps have arrived at without even consulting his sons on the matter, as he then did. It is also somewhat curious in a work of this kind, in which only ascertained fact is dealt with, to find the author, both in his preface and in his conclusion, writing about the docile acceptance of a faith, and commenting on "human perversity which seems to incline some to prefer the Aretic scepticism of negation to the genial warmth of Christian faith," in a manner which, in such a work, seems at least out of place.

AMERICAN WATCHES.

By Prof. JAMES C. WATSON.

Abstract of Report on Horology at the International Exhibition at Philadelphia.

In the examination of the quality of watches produced by the American Watch Company, it became necessary to consider first the mechanical contrivances by which the parts are executed, and then the manner in which these are brought together in the completed movement. While introducing so much that is novel in the way of machinery and processes of execution, there has been no attempt, in respect to the parts of the movements, at innovations which are not of recognized merit. There are certain well-established principles in reference to the proper construction of the train which have resulted from the application of the laws of mechanics, such as relate to the form of the teeth of the wheels, the leaves of the pinions, and proper numbers for each. It is well established also, that for the purposes of a watch to be carried in the pocket, the lever escapement is to be preferred. The results of careful experiments show that when the lever escapement, the duplex escapement, and what is known as the chronometer escapement, are equally well constructed, and placed in movements with equally good trains and equally well sprung, the performance is substantially the same. There being, therefore, no objection to the lever escapement, as regards the time keeping properties, and there being good reasons for its adoption in preference to the two others named, when the manner in which the watch is to be carried is considered, the propriety of this form of construction, even for the finest watches, is put beyond question. There is one innovation upon what was in this country and in England, until recently, regarded as an essential principle of watch construction, which the company ventured upon at the outset. This consisted in dispensing with the fusee and chain, and using in preference the going-barrel, thus reducing very much the size and the complexity of the watch. The Swiss manufacturers long ago ventured, so to speak, to adopt to a considerable extent this form of construction, but the poor quality of the watches which the careless greed of manufacturers and importers flooded upon the markets of the world, did much to strengthen the idea, among the dealers as well as among the purchasers, that a good watch must be provided with a fusee and chain, so that the ultimate force with which the power of the main spring acted upon the escapement should be as nearly constant as possible. Hence the popularity of the English lever watches in this country, and hence the tenacity with which in England they still adhere to the notion that no reliance can be placed upon any other form of construction.

But, wisely, the pioneers in the introduction of this industry into our country, decided to adopt the going-barrel, so as not only to reduce the size and expense of the watch, but also to obviate the great expenses for repairs made necessary by the frequent breaking of the chain. And besides, in the case of the breaking of the mainspring, the recoiling of the barrel, from the sudden removal of the pressure, creates the risk of injury to the teeth of the great wheel and to other important parts of the movement. Yet, in spite of these discouragements, the necessity of a fusee and chain seemed so apparent to those not versed in the knowledge of the action of the balance spring as regulating the movement of the watch, that this notion, so well fortified, did much to oppose the introduction of the watches in which these parts were wanting. Certain it is that many scientific sceptics upon this point had to be convinced by actual trials before yielding their assent to the possibility of the construction of an accurate timekeeper upon the simpler system. It is indeed true that the force of the mainspring is not constant, but if the difference be not too great the isochronal property of the hair-spring, in proper or even approximate adjustment, is quite sufficient to regulate the movement of the watch within the limits required. If the mainspring be of considerable length, and the number of turns when wound

such that only comparatively a few turns are unwound during twenty-four hours, there will be very little variation of the force actually transmitted to the train, so that if the watch is wound regularly, the small differences which would result are counteracted so completely by even an approximate isochronal adjustment of the balance spring as to become practically insensible.

The safety of these simpler movements, from any injury which might result from the breaking of the mainspring, is most effectually provided for in all the watches made by this Company, by a device known as "Fogg's Patent Safety Pinion," invented in 1865. The arbor is tapped with a triple left-hand thread upon which the pinion screws. This pinion is toothed into the great wheel upon the barrel, and the action of the wheel upon it is in the direction of the tightening of the screw. Whenever, therefore, the mainspring breaks, the recoil of the great wheel unscrews the safety pinion from the arbor, and thus instantaneously releases the whole movement from the effect of the reaction.

It is well known that the teeth of the wheels should be epicycloidal in form, and the attempt is always made, in a properly constructed movement, to secure this form in the final process of finishing. Often, however, in the manufacture under the old system, partly on account of the difficulty of securing the proper form, and partly on account of the peculiar æsthetic notions of the workmen who conceive a dislike to epicycloidal teeth because they had in mind the resemblance of their form to that of the bishops' mitres, this important principle was disregarded. Another obstacle arises from the difficulty of producing in miniature with precision what it is possible to draw only upon a magnified scale. But by the application of the machines constructed for this purpose, not only in this instance, but in many other similar cases in the production of the various parts, is the difficulty obviated, and the requirements of theory rendered possible in practice. The forms of the cutters and polishers of the machines which cut the gearing are kept true by means of a machine which gives the true epicycloidal form, so that by proper attention to the depthing when the train of wheel-work is put together in the watch movement, the perfection of smooth and continuous action is secured. It is right here that the superiority of the American system comes into important operation. While it is possible by the ordinary methods, by patient work, to produce the wheels and pinions with the requisite degree of perfection, yet, manifestly, on account of the necessary expense, this cannot be done in the case of the common grades. But by the method under consideration, the action of the machines is precisely the same for all grades. There may be degrees of finish, but the form is unchanged, and the cheapest movements have this excellence in common with the best. The difference of the cost of watches not specially adjusted, will therefore depend upon differences in the number of jewels, the finish of particular parts, and the character of the balance.

The excellence of the train being beyond question, the next and the important consideration is, the character of the work in the construction of the parts connected with the escapement. These are all constructed by processes quite similar to those already mentioned, and hence attention may be directed particularly to the hair-spring and the balance. It is well known that it is upon the proper performance of these parts that the accurate time-keeping property of the movement depends. Any imperfection here will make manifest in a striking degree any imperfections in the train, and even when everything else is perfect, a failure here renders the movement practically worthless for the purpose for which it was intended. The balance spring must be of the very best material, must be evenly coiled, and must be so tempered as to secure a maximum degree of elasticity, and a continuance of this quality undiminished. There will then be a relation between its elastic force, the length to be brought into action, and the weight of the balance which it is to control, to be determined so as to make the vibrations of this balance isochronous whether they be long or short. An examination of the process employed in springing the movements showed that this is accomplished by means of instrumental appliances with all the precision necessary, even in the case of the com-

monest grades of movements. For higher grades, not including those specially adjusted, to be considered hereafter, the process gives results of wonderful accuracy. The symmetry of the action of the spring, as its coils are caused successively to contract or expand, is carefully tested by an indicator constructed for that purpose, and the condition that the center of torsion shall be coincident with the axis of the balance staff, is thus secured, in advance. The elastic force of the spring for different degrees of winding is next determined, and thereupon it is possible to select a balance for an assortment of compensated balances prepared so as to fulfill the requirements for poise and compensation by identical processes, such that the conditions for isochronism are very approximately realized without any special adjustments after actual trials of the running movement. In this way, movements which are of low price, and adapted to the general market, are produced which perform often equal to the most carefully adjusted. An actual trial, by timing tests, of movements of this grade has shown generally only small errors in the several adjustments for temperature, isochronism and position, and in some instances they have been within the limits attainable by special attention to these adjustments in the usual way. Thus often for a very moderate price, indeed, may be obtained a movement of a very high order of excellence, such that if it had been adjusted by the usual method, on account of the attention required from the adjuster, its cost would have been necessarily increased ten fold.

The results of the examination of all the grades so far considered were, as stated in the report for an award, that for watches not specially adjusted, and including all the ordinary and medium grades as to price, this Company is enabled to produce, in the case of such movements, better watches than are produced by the other methods in use, and that these products are worthy of special commendation.

This Company also exhibited watches, the parts of which were also made by machinery, and highly finished, and which had been specially adjusted for isochronism, for position of the balance, and for temperature. One of these, selected at random, was taken down and all its parts minutely examined. The workmanship throughout was found to be excellent, and the materials the very best. The plates were of nickel, the wheels of gold. The holes were jeweled and capped, and the springs of excellent quality, and the balance spring of the form known as the Breguet spring. The workmanship being of a quality to warrant a movement of the highest order, it remained to determine by actual trial the accuracy of the adjustments, and the regularity of the motions from day to day. The limited time available, in the course of the examinations, for a work of this kind, made it difficult to give to these trials proper attention; but the requisite astronomical instruments being in position and available near the United States Government building, (being there as a part of the National exhibit) ten of these movements were taken from among those on exhibition, and subjected to trials. The first trials for errors of position were made for seven of these movements by Mr. Theodore Gribi, a member of the Swiss Commission, whose services as mechanical expert were of great value to the writer of this report in the course of the examinations in the department of horology. The trials made by Mr. Gribi were during days when the ranges of temperature were considerable, the average temperature in the building during the middle of the day often exceeding 100° Fahrenheit. The results of Mr. Gribi's comparisons during a period of ten days, as computed by him, are as shown by the following table: (*See Table on following page.*)

The numbers given show the difference of rate corresponding to a period of 24 hours.

The movement numbered 670052, which was also compared by Mr. Gribi, is, for reasons which will subsequently appear, omitted from this table.

Subsequently, during the trial of the Marine Chronometers, these movements were subjected to further trials, to determine their errors, and especially the errors of compensating for temperature. The results derived by the reporter, from his observations in connection also with the comparisons made by Mr. Gribi, were communicated to the Centennial

No. of Watch.	Dial Up and Dial Down.	Variation Between			Sum of the Four Variations.
		Pendant Up and Pendant Down.	Hanging and Lying.	Pendant Up and Pendant to Right	
	s	s	s	s	s
670087	2.36	8.35	0.11	3.77	14.59
670081	1.40	1.50	2.65	3.23	8.78
670082	0.73	0.98	0.30	0.01	2.02
670083	1.76	0.44	7.01	5.01	14.22
670092	0.88	7.31	3.18	5.19	16.56
570044	3.74	1.45	0.28	0.46	5.93
Mean.....	1.81	3.34	2.25	2.94	10.34

Commission, in explanation of the award reccommended by the Judges for the production by this company of first-class pocket chronometers. These results are as shown by the following table :

No. of Watch.	Variation for ±1° of Temperature.	Difference between Long & Short Arcs of Vibration.	Maximum Error of Position.
	s		
670075	∓0 034		
670099	∓0.188		
670095	±0.024	s	s
670061	∓0.014	2.5	1.4
670087	∓0 038	0.4	8.3
670083	±0.012	2.2	0.5
670052	±0.064	1.3	6.7
670044	∓0.052	0.6	1.4
670082	∓0.064	0.1	0.8
670092	±0.240	0.4	7.0
The Mean of all gives..	±0.07	1.07	3.7

These numbers refer to the variations of the rate corresponding to a period of 24 hours.

In the reduction of the observations for the movement numbered 670052, included in the above table, it was assumed that an abnormal error, shown by Mr. Gribi's comparisons, was due to an error of observation, but subsequently it was found that the movement was not in proper running order, on account of clogging pieces of lint adhering to the escapement, and that it needed overhauling ; and hence in the subsequent trials another movement was taken.

The results thus obtained were sufficient to show that the claim of the production of first-class pocket chronometers was well founded, and they served as the basis for the award to the company for that class of movements. Although sufficient to warrant the award made, it is true they do not depend upon a series of observations, sufficiently extended to be compared fully with the results for the trial of first-class pocket chronometers at the Swiss observatories where such trials are regularly made. And besides, in the temperature trials there was some uncertainty as to the position in which the watches had been placed in the refrigerator during a portion of the 48 hours that they were kept in the cold. In order then, to put the movements to a severer test, they were taken to the Observatory at Ann Arbor, and there subjected by the reporter to a rigorous trial. The whole period of this final trial was eleven weeks, which is five weeks longer than the first-class pocket chronometers are tested at the Swiss observatories. The trial began on September 9th, and ended on November 26th, 1876, and the results of the successive weeks are shown by the following tables (*next page*) :

In these tables (p. 165) the watches are arranged in the order of their merit, as indicated by the range and fluctuations of the rate in all the changes of temperature from the beginning to the end of the trial—not taking into the account the errors arising from the change from the horizontal to the vertical position.

During the seventh week the watches were placed in different positions, with the dial up, in respect to the magnetic meridian, but no sensible errors, which might be attributed to the action of magnetic forces, were indicated.

In the ninth week of the trials the watches were kept during two days at a mean temperature of 36°.7, and then transferred to a warm room in which they were kept for two days at a mean temperature of 95°.1.

During the tenth week, the watches

TABLE SHOWING THE PERFORMANCE OF THE WATCHES DURING A TRIAL OF ELEVEN WEEKS.

Week of Trial.	*First.*	*Second.*	*Third.*	*Fourth.*	*Fifth.*	*Sixth.*	*Seventh.*	*Eighth.*	*Ninth.*	*Tenth.*	*Eleventh.*
Range of temperature	57° to 69°	60° to 72°	47° to 69°	43° to 53°	35° to 50°	39° to 66°	40° to 61°	49° to 65°	31° to 99°	40° to 77°	55° to 69°
Mean temperature ...	62°.1	66°.5	53°.1	48°.7	43°.4	54°.8	48°.8	56°.3	61°.8	52°.7	61°.4
Position.....	Horizontal Dial Up.	Horizontal Dial Up.	Vertical Pend. Up.	Vertical Pend. D'n.	Vertical. Pend. Rig't	Vertical. Pend. Left	Horizontal Dial Up.	Horizontal Dial Up.	Horizontal. Dial Up.	Horizontal. Dial up & d'n.	Horizontal. Dial Up.
No. of Chronometer.	WEEKLY SUMS OF RATES.										
	s	s	s	s	s	s	s	s	s	s	s
670044	− 5.05	− 4.80	+27.55	+52.10	+42.05	+18.20	+ 2.05	− 3.50	+ 1.40	+11.35	− 5.55
670089	+ 3.65	+ 3.35	+47.40	+70.60	+114.85	+66.85	+11.90	+13.50	+12.65	+ 6.40	+10.05
670082	−13.65	− 7.60	+ 9.10	+15.85	+ 9.65	−10.25	− 7.90	−11.30	−11.30	−16.75	−21.95
670095	−20.85	−16.95	−18.15	+ 7.85	+ 1.40	+25.75	−20.90	−16.50	− 8.55	−18.90	−20.00
670087	−14.15	−17.20	+ 6.35	+79.20	+ 67.40	− 1.95	− 8.45	−14.35	− 3.55	− 9.50	−18.85
670083	+ 1.70	+ 7.80	+ 5.15	+57.60	+ 71.95	+42.45	+19.05	+27.95	+23.40	+10.20	+18.20
670090	+ 7.45	+ 6.45	+36.55	+27.30	+ 42.50	+ 1.75	+24.90	+17.80	+29.45	+34.00	+29.80
670061	−27.70	−39.50	+ 1.25	−20.95	− 45.80	−32.50	−47.60	−49.40	−35.70	−36.05	−55.45
670099	−20.25	−26.15	−20.30	+66.15	+ 69.85	+53.25	− 6.25	−18.05	−10.50	− 0.35	−22.65
670092	−15.00	−11.35	−59.43	−31.62	− 63.05	−62.15	−32.45	−31.55	− 9.30	−22.35	− 7.20

These watches are all stem-winders. The duration of trial in each position indicated unmistakably the total effect of irregularities of motion, due to changes of temperature.

were kept three days with the dials down, in order to find the difference of rate between the dial up and the dial down.

By a comparison of the rates from day to day during the periods in which the position remained unchanged, the mean daily variation of the rate of each chronometer was obtained, and the variation of the rate, assumed to be uniform through a considerable range of temperature, corresponding to a variation of one degree of the thermometer, was derived by a comparison of the rates at the extremes of temperature to which they were subjected. In order to exhibit more fully the character of these movements the following tables are subjoined, the numbers in which have been derived from the data obtained in the course of the final trials now under consideration:

(*See Tables on following page.*)

Those who are familiar with investigations of this character will understand, without further explanation, what the numbers in these tables indicate; but for the purpose of making the results clear to the general reader, it is necessary to consider them more fully, commencing with the first of those which immediately precede.

The mean daily rate, as shown in the second column, is the average rate by which each watch gained or lost on mean solar time during the whole period of the trials, and the next column shows the average temperature during the same period. The fourth column exhibits the mean variation of the rate from day to day, excluding the differences which resulted from the changes of position. The amount of this mean daily variation for a particular chronometer is a fair index of the perfection of its manufacture, except so far as relates to the special adjustments next to be considered.

In reference to the numbers given in the fifth column for the variation of the daily rate corresponding to a variation of one degree in the mean temperature, it should be stated that they represent this coefficient as derived from the differences corresponding to a range of temperature from 36°.7 to 95°.1 Fahrenheit, disregarding the effect due to the irrationality of the compensation, for which, in marine chronometers, a secondary or auxiliary compensation is fre-

TABLE SHOWING THE MEAN DAILY RATE IN DIFFERENT WEEKS, AND THE VARIATIONS OF RATE FOR CHANGES OF POSITION.

Number of the Chronometer.	Difference Pendant Up and Pendant Down.	Difference Pendant Right and Left.	Difference of rate between First & Eleventh Weeks.	Difference before and after Vertical Positions.	Mean daily rate for Weeks in which position was horizontal. For temperature 60° Fahr.						
					Before exposure to Heat.				After exp'ure to Heat		
					First.	Second	Sev'nth	Eighth.	Ninth.	Tenth	11th.
	s	s	s	s	s	s	s	s	s	s	s
670090	−1.76	−5.18	+3.19	+0.87	+1.26	+1.56	+2.44	+2.17	+4.39	+4.68	+4.44
670089	+3.14	−6.40	+1.00	+0.51	+0.50	+0.74	+1.25	+1.78	+1.74	+1.38	+1.49
670082	+0.79	−2.38	−1.19	−0.74	−1.87	−0.83	−1.58	−1.77	−1.54	−2.02	−3.07
670095	+3.65	+3.64	+0.12	−0.81	−2.95	−2.33	−3.13	−2.31	−1.20	−2.85	−2.84
670083	+7.09	−3.19	+2.36	+0.01	+0.44	+1.69	+1.71	+3.67	+3.50	+2.35	+2.72
670044	+3.06	−2.27	+0.01	−0.61	−0.63	−0.12	−0.72	−0.83	+0.37	−0.71	−0.66
670082	+4.94	−2.30	+1.24	+0.53	−2.73	−3.04	−2.48	−3.69	−1.74	−1.23	−1.33
670087	+9.94	−9.33	−0.67	−0.18	−1.85	−1.92	−2.10	−2.35	−0.37	−1.93	−2.58
670099	+5.81	−0.43	−0.34	−0.06	−2.54	−2.67	−2.74	−3.21	−1.22	−1.45	−2.99
670061	−3.23	+2.09	−3.96	−1.40	−3.93	−5.55	−6.96	−7.11	−5.08	−5.45	−7.90
Mean, all	±4.34	±3.72	±1.41	±0.57	±1.87	±2.04	±2.51	±2.89	±2.12	±2.40	±3.00

In this table the watches are arranged in the order of their mean daily variations of rate.

The means given at the bottom, show the average accuracy of the performance of the whole group.

TABLE SHOWING THE MEAN DAILY RATE, AND THE MEAN DAILY VARIATION OF THE RATE, CORRESPONDING TO A PERIOD OF TWENTY-FOUR HOURS, IN DIFFERENT POSITIONS, ETC.

Number of the Chronometer.	Mean Daily Rate.	Mean Temperature.	Mean Daily Variation.	Mean Variation for ±1°.	Before and after Oven.	Difference between Hanging and Lying.	Difference Hanging and Pendant Left.	Difference Hanging and Pendant Right.	Difference Dial Up and Dial Down.	Sum of these Four Variations.
	s	°	s	s	s	s	s	s	s	s
670090	+2.76	59.5	±0.12	∓0.098	0.0	−2.98	−5.30	−0.11	−1.30	9.69
670089	+1.15	"	0.14	∓0.038	0.5	−5.75	+2.71	+9.25	−1.77	19.48
670082	−1.75	"	0.20	∓0.041	0.0	−1.83	−2.69	−0.31	−1.55	6.38
670095	−2.47	"	0.21	∓0.014	0.7	+0.26	+6.23	+2.53	+0.10	9.12
670093	+2.34	"	0.26	∓0.089	0.5	+1.27	+5.48	+8.67	−3.25	18.67
670044	−0.38	"	±0.27	∓0.094	0.2	−3.28	−1.16	+1.10	+3.85	9.39
670092	−2.57	"	0.30	∓0.214	0.7	+3.91	−0.74	+1.52	−1.25	7.42
670087	−1.82	"	0.31	∓0.005	0.1	−2.69	−1.13	+8.23	−0.20	12.25
670099	−2.47	"	0.39	∓0.164	0.3	−4.34	+5.01	+5 44	−0.37	15.16
670061	−6.08	"	0.54	∓0.014	0.4	−4.63	−4.75	−6.87	+0.45	16.70
Mean, all	±2.38	59.5	±0.27	±0.077	0.3	±3.09	±3 53	±4.40	±1.41	12.42

In this table the watches are arranged in the order of their mean daily variations of rate.

In trials of this character one of the conditions of competition, often imposed in advance, is that the difference between hanging and lying shall not exceed a certain limit, and the result has been an attempt to accomplish this at the sacrifice of the adjustments in other positions. The movements under consideration were not adjusted specially in reference to any such condition.

quently applied to the balance. In reducing the rates to a mean temperature, whenever such reductions were required, a small correction was introduced on this account, whenever its amount was clearly indicated by the observations.

The numbers given in the column headed "before and after the oven," exhibit the change of the daily rate found by comparing the rates on the days preceding and following the placing of the watches in a heated room, and show how far they are affected by sudden and violent changes of temperature.

In order to understand the significance of the numbers in the succeeding columns which relate to the differences due to changes of position, it is necessary to call attention again to the character of the final adjustments of a first-class pocket chronometer. The general nature of these adjustments and the methods by which they are approximated to by this company, in the case of even the cheaper grades of movements, have already been alluded to, but for our present purpose further consideration of this subject will not be out of place.

The parts of the escapement being supposed to be of proper construction and in proper adjustment in the movement, the attention will finally be directed to the balance and to its controlling spring. In reference to the balance it is often erroneously supposed that the office of the compensation by means of a bi-metallic segmental rim of brass and steel is to counteract simply the effect of the expansion of the balance itself by an increase of temperature, whereas in fact the change of rate arising from the loss of the elastic force of the balance spring, by an increase of temperature, is five times greater than that resulting from the expansion of the balance. And still further there is a change of rate due to the elongation of the spring from the same cause. The compensation of the balance must provide for all these changes, and they amount to a change of the rate to the extent of more than one minute in twenty-four hours for a change of only ten degrees in the temperature. This, however, is now so well understood, that within limits which are always possible to good workmanship, the compensation can be readily effected, and the poise of the balance with reference to the axis of its staff successfully arranged. But when the attention is finally directed to the adjustment of the balance spring, the difficulties to be encountered require the highest knowledge and the most skillful manipulation on the part of the adjuster. On account of the change in the amount of the friction in different positions, and on account also of different degrees of viscosity of the oil at different times, and on account further of inequalities of motion communicated by the train, the arcs through which the balance will vibrate, will be subject to considerable fluctuations even under the most favorable circumstances. When we add to these the interferences which are constantly operating as the watch is carried about in the pocket of the wearer, it becomes of the highest importance that the isochronism of the spring shall be as perfect as possible. The property of an isochronal adjustment, it is well known, is that the vibrations of the balance shall be performed in the same time whether they be long or short. This will require, in general, for a balance of a given weight and diameter, a certain determinate length of the spring in action, and a form such that this action shall be symmetrical in reference to the motion of the balance each way from the point of quiescence. When this adjustment is once secured, it is evident that any change of the acting length of the spring will destroy this important provision; and here comes in one of the difficulties in the adjustment of a pocket chronometer which is to be carried in different positions. In the case of a marine chronometer there is provision made that the instrument shall perform its functions always in the same position, and hence, when the balance is once poised, regulated to time, and the isochronal adjustment of the balance spring completed by comparing the times of vibration, or the daily rate, in long and short arcs, obtained by varying the acting power of the mainspring, the desired result is accomplished. But in the case of a watch, even if the poise be perfect, the isochronal adjustment made perfect for a horizontal position, will be found to be in error in other positions because of the modification of the action of the spring as its position is changed. It becomes a very difficult matter, there-

fore, to secure the perfection of the adjustments for a variety of positions when an extreme limit of precision is sought. It has already been mentioned how these adjustments are by instrumental appliances effected in all the watches made by this company, when once the required relation of the parts has been established; but the production of movements of the highest degree of excellence requires that further and special adjustments be made.

The elastic force of the spring, as its coils are contracted or expanded from the state of quiescence, must change its value in a ratio depending upon the weight and diameter of the balance, for the case of isochronal action, and this relation is found by careful trials. The weight of the balance is not affected by the temperature, but its diameter is thus changed, and here again a source of error creeps in. It is the accumulation of all these errors, infinitesimal almost in each vibration, which, in the course of the twenty-four hours, gives a finite error in excess or defect, and thus alters the rate of the watch. If the acting part of the spring be too short the tension as it is wound will be too great, and in the course of long arcs of vibration the rate will gain, while in short arcs it will lose. But if the acting part be too long the tension due to the elastic force will be too small, and the watch will lose during the long arcs of vibration and gain in the case of the short arcs. Any bend in the spring changes the effective length in action, and hence the final isochronal adjustments are effected by modifying the terminal curves of the spring without unpinning it from the collet or the stud.

It is evident further that when once these adjustments are effected, it is of the greatest consequence that the conditions remain unchanged. Hence the importance of a spring properly and permanently tempered. It has been established by experiment that the effect of the process of tempering the spring is to leave it, before it has been put into use, in a condition such that the relations of its molecules do not become permanent until it has been for some time in continuous vibratory motion, and further that the effect of such motion is to produce in the outset an increase of its strength and elasticity, and hence an acceleration of the rate of the movement which it controls.

The use of the flat spring in these chronometers requires that, for the best and most permanent adjustment, a terminal curve for the spring be brought out of and over the plane of its coil. This is not essential for a spring whose action shall be always in the same position, but when the positions are to be different the form of this terminal curve performs an important office in the adjustment. Sometimes both ends of the spring are turned into such terminal curves, and theoretically there would seem to be good reason for this arrangement; but the difficulties of finding, in practice, the proper forms for two such terminal curves, is a sufficient warrant for confining the attention to a single curve, as in the Breguet form of the spring adopted in the construction of the watches under consideration.

The accuracy of the adjustments depending upon the positions in which the watches are placed, are sufficiently indicated in the several columns of the tables. A reference to the table showing the weekly sums of the rates, will indicate the time allotted in the trials to each position in which the movements were tried. It will be observed that the periods were such as to make prominent the irregularities depending upon these changes of position, and further that the trials extended to six positions, besides tests for the influence of possible magnetic action. It is hardly necessary to add that the tables establish conclusively the excellence of all the adjustments. And it should be borne in mind, in a just determination of the merits of these movements, that they were subjected to great vicissitudes before they received the long trial of eleven weeks. They had been taken from the factory to the Exhibition before its commencement. They were not regularly wound and kept running, but only so kept during the progress of three distinct trials separated by considerable intervals, and they were exposed to great changes of temperature. After the partial trials at Philadelphia they were allowed to remain unwound and at rest, then they were again wound and started, carried to Ann Arbor, and after an allowance of

only three days to assume their regular rate, the final trials began. These extended over the long period of eleven weeks, and through considerable ranges of climatic temperature, circumstances such as to test severely their running qualities. In order to do full justice to the exhibit, it would have been better to have extended the trials to a much larger number of movements, because in the case of so small a number involved in the means taken, a single watch going badly, or not very closely adjusted for errors of temperature and position, affects the mean results much more than if the number were greater. The limitation of the number was not the fault of the exhibitors, but was determined by the convenience of the reporter, who could not undertake, in connection with the other duties devolving upon him, the trial of a greater number.

It would be of benefit to the company if their finest productions could be subjected to trial every year in some astronomical observatory. Nothing has done more to stimulate the Swiss manufacture toward excellent workmanship and the most careful adjustments possible, than the competitive trials which have been made for a series of years at the Observatories at Neuchatel and Geneva. In this way, by comparison of results, can some idea be formed of the progress made and the degree of perfection attained. The trials of the first-class pocket chronometers at the Swiss observatories extend over a period of six weeks. They are tried in five positions and in the oven. The second grade of pocket chronometers are subjected to trial for four weeks, in two positions, hanging and lying, in the oven. It will be seen by reference to the parts of this report which refer to the English, Swiss and German exhibits, that the labors of the Judges were very much facilitated by the certificates issued from the observatories, showing the results of the actual trials of the adjusted movements which were on exhibition. And in a determination of the progress which has been made in America, there was thus provided a standard of acknowledged excellence. It is not the purpose of this report to make direct comparisons of the productions of different manufacturers. The reader who is curious in such matters will find, in most cases, the requisite data under the heads of the report relating to the exhibits in question. But in order to convey to the general reader an idea of what the system of manufacture under consideration has made possible in a newly developed industry in this country, it is proper to state here that the reporter has compared the results of the trials as heretofore given with those furnished for all first-class fully adjusted watches in the International Exhibition, and that it is clear beyond question that the chronometers numbered 670044, 670082 and 670095 are altogether superior to any others exhibited. The tables already given show clearly the excellence of adjustments, and the smallness of the mean daily variation of their rates. The best of these, taking everything into account, is No. 670044, for which the mean daily rate was only thirty-eight one-hundredths of a second, and for which the difference of mean daily rate between the first and eleventh weeks of the trial was only one hundredth of a second. The steadiness of the rates of these watches is best shown by placing in juxtaposition the average daily rate during several weeks separated by considerable intervals, and during which the determining conditions were very different, as already stated. The following are the results:

Week ending.	Mean daily rate of		
	No. 670044	No. 670095	670082.
	s	s	s
1876. July 20.	—0.50	—3.60	—2.80
Sept. 18.	—0.63	—2.95	—1.87
Oct. 30.	—0.72	—3.13	—1.77
Nov. 26.	—0.66	—2.84	—3.07

These numbers, for the sake of comparison, are reduced as nearly as possible to the temperature 60° Fahrenheit.

The performance of No. 670044 is extraordinary, considering the vicissitudes to which it was subjected and the long period of nineteen weeks during which its rate is considered. The mean daily variation of the rate is greater than that of each of the other two, but this may be due to the imperfect elimination of the error of the eccentricity of the second's dial, in the daily comparisons. Its temperature correction is also appar-

ently somewhat larger, but that all the essentials for good performance were present, is best attested by the steadiness of the rate from week to week as it was under trial.

No. 670089 is hardly of inferior rank to the three here specially mentioned, although the sum of its position errors is greater than the general average.

The results thus developed show clearly enough that there is no lack, in this country, of mechanics skilled in the most abstruse principles of horology, and able to execute their work in accordance therewith. But it is a lamentable fact that the great majority of those who are to care for these productions after they are put into use, are in total ignorance of the fundamental principles upon which the performance of a watch depends. It would be of inestimable benefit to the public, from an economic point of view, if there could be established in this country schools of horology where young men desiring to become experts in the repairing even of watches, could be enabled to acquire that knowledge and experience which is essential to the proper performance of this kind of work. In every industrial or intellectual pursuit in life, it is desirable that men should be thoroughly educated in the principles underlying their profession, and in none more so than in that which we are considering. It is true that we find occasionally men in this profession, also, imbued with the desire to know the why and the wherefore, who have sought to acquire, and often have acquired such education; but too often indeed do we find men professing to be accomplished watch repairers who are in profound ignorance of the principles upon which the peculiar functions of these instruments are correctly performed, and who are liable to undo in a moment what has been achieved by the patience and skill of an accomplished manufacturer. The reader will already have in mind, from what has been under special consideration, that the greatest care is necessary in respect to fully adjusted watches. For needed repairs they should be entrusted to men who fully understand that the relations of the balance and the hair-spring should never be disturbed, even to the minutest bending or lengthening of the spring, or disturbance of the weights of the balance.

It is apparent, therefore, how important it is to the possessor of a good watch that it shall not be liable to be ruined by the ignorant manipulation of some professed watch repairer who undertakes to clean it. The desire to regulate such a watch to run in conformity with a worthless clock, with pretended compensation of the pendulum, has often been the source of mischief to the machine which should regulate instead of being regulated. And further, it should be borne in mind, that the use of watches has now become so general that the expenses of cleaning and repairs must amount to millions of dollars annually. These expenditures result partly from necessity, partly from the worthlessness of movements palmed off by irresponsible dealers upon a confiding public, and partly from the ignorance of those who undertake the repairs. It is one of the merits of the system of watch manufacture under consideration, that it reduces to a minimum the expenses to be incurred and the dangers to be encountered.

The general reader unacquainted with the results which are obtained from clocks and chronometers of the best construction, by those who are concerned with the accurate determination of time, may perhaps expect closer results than those indicated in the foregoing tables, especially when recalling to mind some of the extravagant statements often made as to the running qualities of watches in the hands of wearers whose comparisons have been made at long intervals, and often with uncertain standards, thus obtaining accidental coincidences. The trial of the movement from day to day, with exact standards of comparison, will necessarily reveal those errors which are compensated or concealed when the intervals of comparison are very long.

The fullness of this report on this particular exhibit has been made necessary, on account of the disposition manifested by certain foreign manufacturers to arrogate to themselves and their country the sole possession of that knowledge and skill which can render possible the higher achievements of horological art. While conceding, as they have been com-

pelled to do, by the inexorable logic of the facts before them and the world, the superiority of the American system of manufacture in the case of all but the specially adjusted watches, they have been disposed to cling to the latter as of their exclusive control, and to look with suspicion upon any results which went to show the possibility of excellent production in these higher grades also. The final adjustment of a watch for position, for isochronism, and for temperature, requires only that the maker shall be able to produce the best quality of springs, of the desired form, and to make a balance of proper proportions, it being supposed that the escapement and the train have been properly constructed. The adjuster must understand the methods of springing, must know how to secure the poise of the balance, and how to modify the acting part of the spring so as to secure isochronal action in different positions and at different temperatures. It cannot be supposed that the science and skill which have achieved wonders in all of what might be called the inferior operations, should find an insurmountable barrier here. The results of the trials of the very few watches selected for that purpose, from among those of the exhibit under consideration, show that such is not the case. And the manufacturers may claim superiority for their finest productions, for the very simple reason that the machine to be adjusted, finally, for all the errors which interfere with accurate time keeping, is a better machine when constructed upon their system than when constructed upon the old system, and the better machine being equally well adjusted with the inferior one, in respect to these isochronal functions, must, in the nature of things, be of a perfection more enduring, and must satisfy better all the conditions for the highest productions of horological art.

CABLE-MAKING FOR SUSPENSION BRIDGES AS EXEMPLIFIED IN THE EAST RIVER BRIDGE.

By WILHELM HILDENBRAND, C. E.

Written for Van Nostrand's Magazine.

I.

The great interest, which American and foreign Engineers have taken in the construction of the East River Bridge, concentrates itself justly in the manufacture of the wire cables. It is acknowledged to be the most difficult part of the construction, an unusual engineering task and a work which, invented and patented by the late John A. Roebling, has been executed so far only by him and his son, the present Engineer. These facts, combined with the circumstance that the method is not generally known, have induced the writer to extend this article, originally intended to contain only some theoretical investigations, and to give, with the kind permission of Chief Engineer Col. W. A. Roebling, a detailed description of the cable-making machinery.

This will include a narrative of the course of construction, as far as finished.

He would feel satisfied, therefore, if he should succeed in contributing in the following pages something to the general information about this great work.

In manufacturing wire cables, there are three different methods in use :

First, the cable is composed of twisted wire ropes.

Secondly, it consists of parallel wire strands, previously made on shore and then hoisted or hauled in position, or

Thirdly, it is made right in place; each wire is taken over separately and adjusted; and all of them are by a wrapping combined into a cable.

The last method will be followed at the East River Bridge, and is the same which was used by the late Mr. Roebling for constructing the cables of the Niagara, Cincinnati, Alleghany and other suspension bridges.

Each method has advantages, which in

certain cases will recommend its application. A wire rope is easily handled and a cable of ropes can be formed quickly and without the aid of machinery. In small or light bridges therefore, this kind of cable is found to be most advantageous. But the tensile strength of a straight wire is ten per cent. larger, than of one twisted; hence, in large bridges a rope cable in the first place would not be economical, requiring more material and costing more per pound than one formed of straight wires. Secondly, the bulk of the former will exceed the latter by forty per cent., offering therewith so much more surface to the wind and to the corroding action of the atmosphere. This is very important, and would in case of the East River Bridge, which is exposed to great gales and to salt water air, alone decide in favor of a cable of straight wires. Finally there is great difficulty in making good attachments of heavy wire ropes with the anchor chain, which at best will always be clumsy and require more extensive masonry than the compact and neat wire connection. A cable of wire ropes for the East River Bridge, consequently must be rejected.

Considering now the second method (which, for instance, was used at the Wheeling Bridge), we perceive, that it is only applicable, if in the line of the bridge behind either anchorage there is disposable room of the length of the cable. To manufacture the strands somewhere else and transport them in place is not possible, because a strand of straight wires can not be handled like a rope, and coiling it up would render it useless.

It is evident, that in the present case, where the bridge connects two crowded cities, such available room does not exist, and, therefore, that necessity already compels us to exclude this method. But even if the location would favor it, there are other reasons, which speak against strand-making on shore. When a number of wires, laid up straight without tension, are tied together and suspended in a curve, those in the lower part are considerably higher taxed than those in the upper. This difference of tension in the single wires may amount to a loss of twenty-five per cent. in total strength. Through the elasticity of the material a certain equalization will take place, but nobody can tell to what extent, and the determination of tension in a single wire can only be hypothetical. Furthermore, it is no easy matter to handle a strand of fifty tons weight, which will exert a strain of forty tons, while being pulled over the towers and put in position. All points so far are decidely in favor of the third plan. But one great disadvantage is connected with it, namely, the loss of time involved, from the fact that towers and anchorages must be finished, before cable making can commence. This is not the case with the first two methods; wire ropes or wire strands can be manufactured while the masonwork goes on, and after completion of the latter, they can be put in place at once. The advantages of making the cables in place are, however, as we have seen, so predominant, that there could be no hesitation in adopting this method at the East River Bridge. It is hardly necessary to mention here the impossibility of moving the entire cable, finished, in its place, which opinion seems generally to prevail among non-professional people. Not to speak of the impracticability of its manufacture, the consideration—that it is 3577 feet long and weighs 870 tons, shows, that insurmountable obstacles would resist the locomotion of such a mass and that no tower could withstand the side thrust caused by the friction in taking the cable over.

Before going into details, I will premise by a description, in general outlines, of how the cables are made, using therein the dimensions and names of the East River Bridge. This will facilitate the understanding of the cable machinery and will also be applicable to any other suspension bridge by changing dimensions and names according to its size and location.

The floor of the East River Bridge, which is eighty-five feet wide, and destined to carry all kinds of traffic including railway cars pulled by endless ropes, will be supported by four cables. These are suspended in three spans : a middle or river span of 1595.5 feet between centers of towers, and two side or land spans each 930 feet from center of tower to face of anchorage or 954½ feet to point of cable attachment. Each cable

consists of nineteen strands of 332 parallel steel-wires and contains therefore altogether 6308 wires, which represent a total ultimate strength of 10,730 tons. Each strand is secured with a 7 inch pin of iron to two anchor bars $1\frac{1}{4} \times 9$ inches. The wires do not pass around the pins directly, but around a cast iron shoe, which rests against the pin and which increases the curve of bending from 7 to 17 inches diameter. The last link of the anchor chain, to which the strands are attached, is arranged in four tiers. Each of the three lower ones is destined to hold five strands, the upper only four. While the strand is being made, it does not occupy the position it ultimately will have in the cable, but it hangs at an elevation considerably above. This difference in height, which in our case amounts to 55 feet in the center of river span, is produced in two ways: first, on the towers the strand rests on rollers above the saddle and at the anchorages, the above-mentioned shoe is temporarily secured 10 or 12 feet behind the anchor pin to a casting called "the leg", which is specially designed for this purpose.

After the strand is finished, the shoes are relieved from their seats on the legs and let forward into their places on the anchor bars; at the same time the strand is lowered from the rollers on top of the saddle into the saddle, which double operation causes the vertex to sink in the correct position, previously determined upon by calculation.

There are various reasons for making the strand in a more elevated position. It is clear out of the way of the main cable; the latter does not interfere with regulation, which otherwise would be the case and which would delay the operation considerably. The flat curve, in which the wires are suspended, facilitates the tying together, and the separate attachment of the shoe gives ample working room for laying the wires into it. But the main advantage is derived from the fact that the tension in the wire is nearly doubled, amounting to about three quarters of the maximum tension to which it ever will be subjected in the finished bridge. This tests the wire to a certain extent, takes out all waves and bends, and leads to the easier detection of a defective wire or splice. It gives, therefore, to the engineer more or less assurance that all wires, worked in the cable, come up to the requirements and that the latter will obtain its calculated strength.

The running out of the wires takes place from the Brooklyn anchorage, where all wire rings are received. A number of them, spliced together, are wound on wooden drums, which are large enough to contain about 12—14 wires of the length of the whole cable. The end of a wire, taken from the drum, is now temporarily fastened to the leg, and the loop, formed in this way, hung into a grooved wheel called "traveling sheave," which is firmly attached to an endless rope stretched from anchorage to anchorage. The latter, called "traveling or working rope," passes at each anchorage around horizontally placed wheels which, connected with a steam engine, give motion to it. The traveling sheave with two wires are carried over by the rope. One wire, which is fastened to the leg appears to stand still, while the other, which unwinds from the drum, runs with twice the speed of the working rope. After their arrival at the New York anchorage, the wires are taken from the sheave and laid around the shoe in such a manner, that all standing wires occupy one side of it, and all running wires the other. These two wires are now regulated according to a "guide-wire" which previously has been suspended and adjusted to the desired deflection. The same operation is repeated 166 times, until all wires for one strand are stretched. The regulating is done by men standing on light platforms called "cradles," which, supported by wire ropes, are erected at different places along the line of the cable. It is therefore necessary, that these cradles should be at such an elevation, that the wires of the strand hang about breast high to a man standing in them, so as to enable him to compare the relative deflections of the wires.

In order to reach the cradles without difficulty and otherwise to facilitate general communication between all parts of the work, there is a narrow and light "footbridge," spanning the river and the spaces between towers and anchorages, which serves for all purposes. The wires of a strand are tied together with a wire

lashing about every sixteen inches, and then lowered in their final position. A second strand is made in the same manner, and so on until with the nineteenth the cable is ready for wrapping. The strands occupy in the cable a certain fixed order, and are regulated among themselves in shortening the longer ones by the insertion of an iron segment between shoe and anchor pin. All cables of the large suspension bridges, built so far, never contained more than seven strands; these of the East River Bridge are the first, which will be formed of nineteen strands. The number 7 or 19 is chosen for the reason, that the cable shall form a cylinder and a section through it shall reveal the strands in an order, that a circle can be drawn around them as tangent, (see Fig. 5.) Between 7 and 19 there is no number, which makes this possible.

After all the strands are regulated, the temporary lashings, which held the wires together, are removed, powerful screw squeezers press them in cylindrical shape, and a wrapping of No. 10 wire is put on, which finishes the cable. Figs. 1 and 2 show the position of cradle ropes, cradles and foot bridge, in elevation and ground plan; also the position of the wire-drums, anchor links, working rope and a wire, while traveling across. The guide wires, or the strands in the high position, are only indicated in the ground plan in order not to confuse the drawing. In the elevation they would hang parallel to and a little above the cradle ropes.

The general description given above, shows us that for making a wire cable the following permanent and auxiliary structures, appliances and materials are necessary:

I. Anchorages for attachment and towers for support of the cables.

II. Auxiliary structures and appliances:

1. Means and machinery for taking wires over. Traveling rope, etc.
2. Certain places along the line of the cable, from where the wires can be adjusted. Cradles, etc.
3. Means of access to these cradles. Footbridge.
4. A preliminary, carefully adjusted wire to regulate the others by,—guidewire.

III. The cable wire and method of working it into a cable.

1. The material and its treatment.
2. Regulation of wires and strands.
3. The wrapping of the cable.

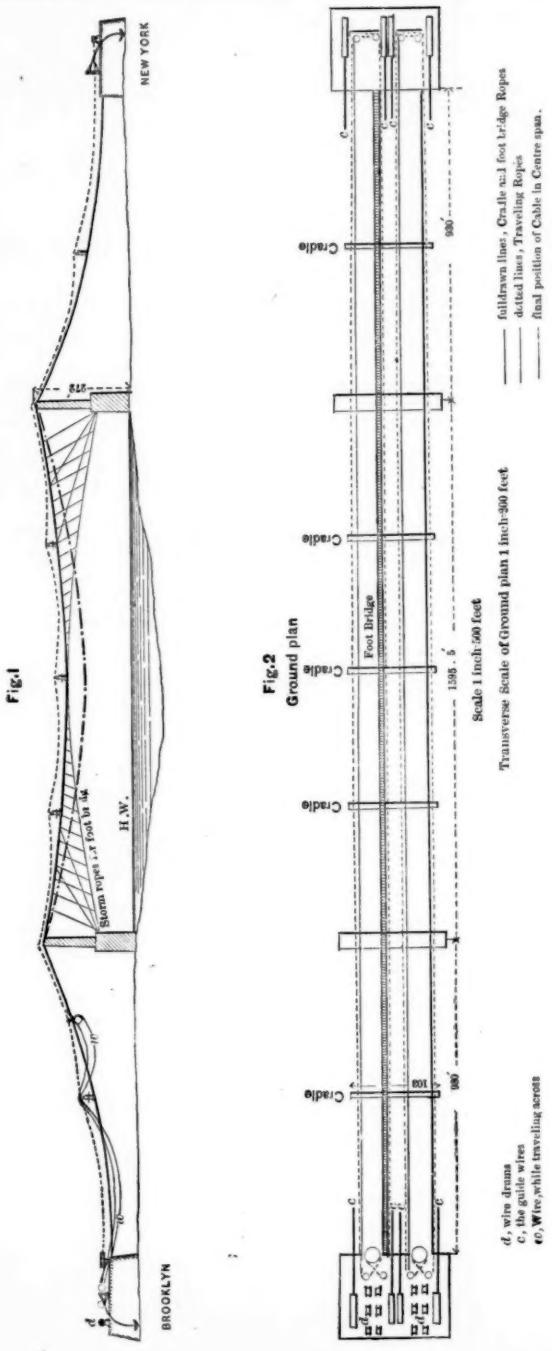
All under heads I and II must be completed before cable-making can begin. The work at the East River Bridge was commenced on Dec. 26, 1869, when ground was broken for building the caisson for the Brooklyn tower foundation.

In August of 1876, after various interruptions, towers and anchorages were completed, and the first rope (one of the traveling ropes) was taken across the river. During the previous winter all machinery pertaining to traveling ropes, etc., was erected on the Brooklyn anchorage. Building cradles and footbridge, suspending the guidewires and other preparations required three-quarters of a year longer, until on May 29th, 1877, the first wire for actual cable-making was stretched. It is estimated, that it will require from 2 to 2½ years to complete the four cables.

I. TOWERS AND ANCHORAGES.

The building of the lofty stone towers and the massive anchorwalls, works of unsurpassed magnitude, has already been extensively described in various reports and scientific journals, so that I can confine myself to the repetition of some principal dimensions, especially as far as they have bearing on cable-making.

The towers, containing 40,000 cubic yards of masonry each, are built throughout of granite, coming from over twenty different quarries of Maine. Their dimensions are at high water line 140 by 59 feet, and on top below the cornice 126 by 43 feet. The summit is 272 feet, and the saddles in which the cables rest, 267 feet 6½ inches above high water. The cast iron saddles, (see Figs. 39 and 40), weighing 12 tons each, rest on 43 iron rollers of 3½ inches diameter, which are moveable on a cast-iron bed-plate, 4½ inches thick and weighing 11 tons. On the flanges of a saddle, six short stands are cast, serving as bearings for three rollers, in which the strand rests during its construction. The middle roller is placed 6 inches outside the center of saddle, the saddle


NEW YORK
BROOKLYN
H.W.
Storm rope 1.x foot in dia
Fig.1
Fig.2
Ground plan
Cradle
Foot Bridge
1595.5'
Scale 1 inch 300 feet
Transverse Scale of Ground plan 1 inch 300 feet
d, wire drums
c, the guide wires
e, Wire, while traveling across
full drawn lines, Cradle and foot bridge Ropes
dotted lines, Traveling Ropes
final position of Cable in Centre span.

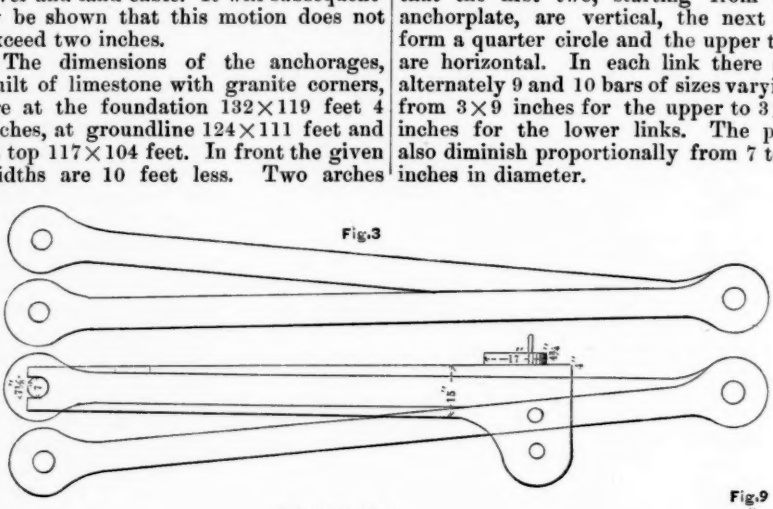
7 inches beyond center of plate, and the latter 12 inches outside the center of tower, all in direction towards the land.

This was done as a precaution, to provide for the probable motion of the saddle towards the river, and to prevent the resultant of pressure from intersecting the tower base at foundation further outside its center than it naturally does, owing to the difference of inclination of river and land cable. It will subsequently be shown that this motion does not exceed two inches.

The dimensions of the anchorages, built of limestone with granite corners, are at the foundation 132×119 feet 4 inches, at groundline 124×111 feet and at top 117×104 feet. In front the given widths are 10 feet less. Two arches run lengthwise through the masonry, dividing it in three piers, in which the anchor chains are bedded. The total height of the anchorage above high water is at the face 89 feet from where it falls towards the rear with 3¼ per cent.

The anchor chains consist of a double set of links, placed over each other. Each set contains ten links, arranged so, that the first two, starting from the anchorplate, are vertical, the next six form a quarter circle and the upper two are horizontal. In each link there are alternately 9 and 10 bars of sizes varying from 3×9 inches for the upper to 3×7 inches for the lower links. The pins also diminish proportionally from 7 to 5 inches in diameter.

Fig.3



Scale ⅛ inch=1 foot

Fig.8

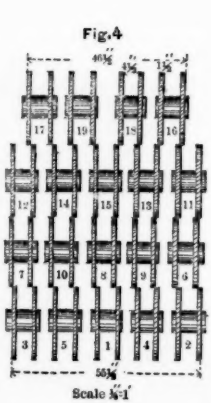
Fig.9

Fig. 3 shows in side elevation the upper set of anchorlinks to which the strands are attached. The figures (Fig. 4) written between the bars indicate the order in which the strands are made in succession.

About 10 or 12 feet forward of the shoe, the two halves of a strand are combined into one, and all strands, before leaving the masonry, are squeezed into a round cable, in which they occupy the following position: (see Fig. 5).

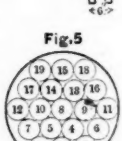
The cables as solid cylinders emerge from the anchorage eight feet below grade line of bridge.

Fig.4

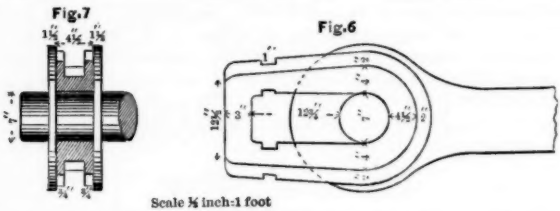


Scale ⅛=1'

Fig.5



⅛" = 1 foot



An important part in anchoring the strands is the "shoe" (see Figs. 6 and 7). It should combine strength with the greatest compactness in order to reduce the width of the chain to a minimum. To resist the squeezing pressure of the strand the shoe is stiffened in front by a connecting piece; but enough length should be given to enable regulation of the strand. The advantages of the shoe are various. It increases the diameter of the curve around which the wires are bent, and it holds the latter between its flanges firmly, preventing any slipping after a wire is once regulated. In removing the strand from its temporary to its final position, the shoe especially is of great advantage, as all connections for hauling are made with the shoe, saving thereby the wires from injury or displacement.

The shoe in its first position rests on the so-called "leg," of which a description may be in place here, as part of the anchorage, though being only of temporary use :

The "leg" is a trough-shaped casting about 12 to 13 feet long. It is held in front by the anchor pin and has in the rear a half round block cast to it for the shoe to rest against. Fig. 3 shows the side elevation of the leg in its position on the anchorbar, and Figs. 8 and 9 give ground plan and rear view. In order to prevent its rear end from rising through the tendency of the strand to draw upwards, a pin connects the two sides which straddle the bar.

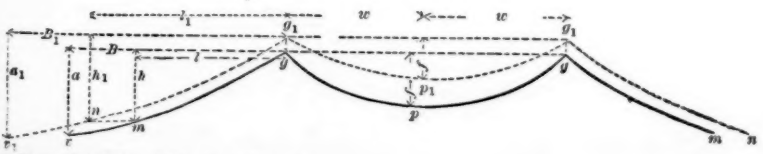
An important point in designing the leg is the determination of its length. This can be done best and surest by actual trial, in manipulating with the guidewire reversedly as afterwards with the strand. The guidewire is lifted from the saddle into the saddle rollers and both ends are pulled back until it has reached the elevation in which the strands shall be made. The distances for which the ends were pulled back give the length of legs. However, it is often desirable to have the legs cast before all other appliances are in readiness, in order not to delay the work. In such case this length must be determined by calculation.

The problem therefore is the following :

A wire, fastened at mm and supported at gg, is suspended with a deflection f. It is supposed that no friction exists at points g; hence the three parts hang in perfect equilibrium, and the tension in the wire on both sides of g is the same. Now the supports g are raised to g_1, and the points m moved horizontally to n, which causes the wire in Fig. 10 to take the position of the dotted curve with a fixed deflection f_1. The distance $m\ n$ shall be determined. It will first be necessary to calculate the length of the whole wire in its first position, or, as the curve is symmetrical to the center line, half its length : $\overline{mg}+\overline{gp}$.

If the wire were to consist of perfect homogeneous material, it would hang in a catenary. This not being the case, we have a right to assume that the weight is equally distributed over the

Fig.10



horizontal projection of the curve, or, in other words, that it forms a parabola. This assumption is the more justified by the consideration, that the future cable is uniformly loaded so heavily that the differences in own weight, calculated per foot of its horizontal projection, are vanishing.

Hence point m is a point of a parabola, which, if prolonged, would have its vertex in v. Taking point v as origin and calling the coordinates of g: a and B, those of m: $(a-h)$ and $(B-l)$, we have as first condition:

$$\frac{B^2}{a}=\frac{(B-l)^2}{a-h}. \qquad (1)$$

The equality of the tensions on either side of g, gives the second condition:

$$\frac{\sqrt{B^2+4a^2}}{2a}Bq_1=\frac{\sqrt{w^2+4f^2}}{2f}wq$$

q and q_1 signify the weight of the wire per unit of length in center and side span. The actual tension being of no importance, we assume q equal to 1 and q_1, in consequence of the steeper curve equal to 1.01.

w=half of center span=799.65 ft.

f=deflection=121 feet.

The right side of above equation therefore is equal to a constant T, and we can write:

$$\frac{\sqrt{B^2+4a^2}}{2a}Bq_1=T \qquad (2)$$

Through the solution of equations (1) and (2), we find the unknown distances a and B.

It follows:

$$a=\frac{B^2h}{2Bl-l^2}$$

$$B=\frac{l^2}{2(l^2+h^2)}+\sqrt{\frac{h^2T^2}{q_1^2}-\frac{l^2}{4}+\left(\frac{l^2}{2(l^2+h^2)}\right)^2}$$

The values for l, h and T in our case are:

l=952.55, h=188.3 T=2760.8,

which, if introduced, give:

a=188.48 B=983.3

With the position of the vertex v, established through a and B, we are now able to express the length s of the curve $\overline{mg}+\overline{gp}$ in a formula:

$$s=w\left\{1+\tfrac{2}{3}\left(\frac{f}{w}\right)^2-\tfrac{2}{5}\left(\frac{f}{w}\right)^4\right\}$$
$$+B\left\{1+\tfrac{2}{3}\left(\frac{a}{B}\right)^2-\tfrac{2}{5}\left(\frac{a}{B}\right)^4\right\}$$
$$-(B-l)\left\{1+\tfrac{2}{3}\left(\frac{a-h}{B-l}\right)^2-\tfrac{2}{5}\left(\frac{a-h}{B-l}\right)^4\right\}*$$

$a-h$ being very small, the last member simply can be called $B-l$ without committing a great error. All the values introduced, we find

$$s=1787.74 \text{ feet.}$$

The wire in the upper portion forms the curve $n\,g_1\,p_1$, the length of which also is equal to s (neglecting thereby the elongation to which the wire is subjected under the greater tension), hence:

$$w\left\{1+\tfrac{2}{3}\left(\frac{f_1}{w}\right)^2-\tfrac{2}{5}\left(\frac{f_1}{w}\right)^4\right\}$$
$$+B_1\left\{1+\tfrac{2}{3}\left(\frac{a_1}{B_1}\right)^2-\tfrac{2}{5}\left(\frac{a_1}{B_1}\right)^4\right\}-(B_1-l_1)=s \qquad (3)$$

In this equation are three unknown values a_1, B_1 and l_1, consequently, to solve the problem, we need two other equations. They are found by expressing in symbols, that the tension of both curves must be alike, and that n is a point of the parabola $\overline{v_1g_1}$:

---

* The above formula gives the length only approximately. The mathematically correct formula is the following:

$$s=\frac{p}{2}\left\{\sqrt{\frac{2f}{p}\left(1+\frac{2f}{p}\right)}+\text{log. nat.}\left(\sqrt{\frac{2f}{p}}+\sqrt{1+\frac{2f}{p}}\right)\right\}$$
$$+\frac{p_1}{2}\left\{\sqrt{\frac{2a}{p_1}\left(1+\frac{2a}{p_1}\right)}+\text{log. nat.}\left(\sqrt{\frac{2a}{p_1}}+\sqrt{1+\frac{2a}{p_1}}\right)\right\}$$
$$-\frac{p_1}{2}\left\{\sqrt{\frac{2(a-h)}{p_1}\left(1+\frac{2(a-h)}{p_1}\right)}+\text{log. nat.}\left(\sqrt{\frac{2(a-h)}{p_1}}+\sqrt{1+\frac{2(a-h)}{p_1}}\right)\right\}$$

in which $p=\frac{w^2}{2f}$, $p_1=\frac{B^2}{2a}=\frac{(B-l)^2}{2(a-h)}$

For flat curves both formulas are almost identical. In our case the error is less than $\frac{3}{1000}$ of a foot.

$$\frac{\sqrt{B_1^2+4a_1^2}}{2a_1} B_1 q_1 = \frac{\sqrt{w^2+4f_1^2}}{2f_1} wq = T_1 \quad (4)$$

$$\frac{B_1^2}{a_1} = \frac{(B_1-l_1)^2}{a_1-h_1} \quad (5)$$

In the last three equations is :

f_1=66ft., $h_1=h+\overline{gg_1}$ =190.42 T_1=4909.8 w_1, q and q_1 have the former values.

From the introduction of these values, follows :

B_1=1406.05, a_1=210.07 l_1=964.65.

hence

$\overline{mn}=l_1-l$=964.65−952.55=12.1 feet,

which represents the length of leg. Owing to a somewhat larger stretch, than was anticipated, in the cradle ropes, the above deflection of 66 feet was a little increased, in order to hang the strand most conveniently for the regulator.

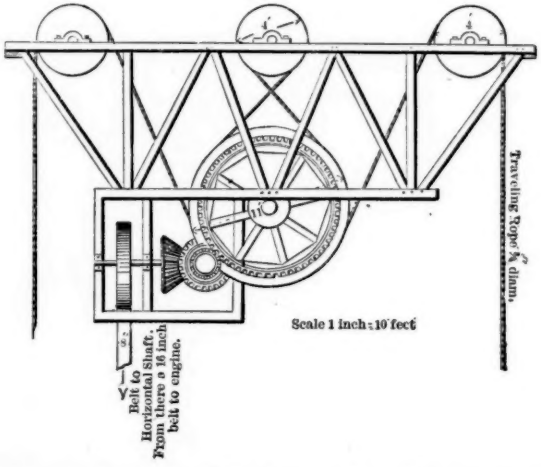
This necessitated a shorter length of leg than calculated, which finally was fixed to 11 feet 10¾ inches.

II. AUXILIARY STRUCTURES AND APPLIANCES.

1. *Traveling rope.*—The "traveling or working rope" or "traveler" consists of a ¾ inch steel wire rope, which forms an endless line around certain wheels placed on each anchorage.

Fig. 11 represents the arrangement on the Brooklyn side. The rope makes first two turns around a double grooved wheel 11 feet in diameter, called "driving wheel" and then passes around two smaller ones, called "guiding wheels," which keep the ropes in the desired distance from each other. On the New York side two similar wheels do the same service, which are fixed in a movable frame, so as to enable taking up slack in the rope. The driving wheel has a gearing on its circumference in which a pinion works of 15 inches diameter. On the pinion shaft is a mitre wheel in which are matched two others, both worked by a lever to throw them in or out of gear. The motion of the driving wheel and working rope can by these means be reversed. A 3 feet pulley on the shaft of these bevel wheels receives its motion by an 8 inch belt from another horizontal shaft which runs along the front face of the anchorage and which, by a 16 inch belt, is in connection with a steam engine located in the yard below. The 8 inch belt runs loosely on the pulley and receives adhesion by tightening pulleys which enable the working rope to be set in motion or stopped, without stopping the engine. The engine has a 12 inch cylinder with

Fig. 11

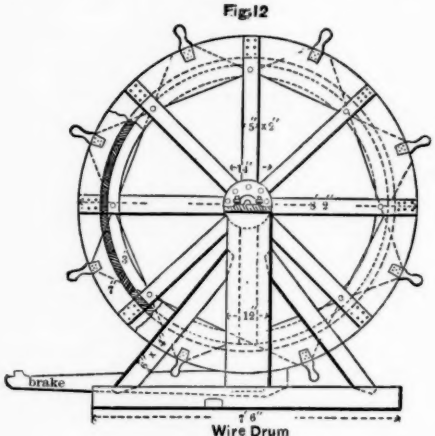


24 inches stroke, carries from 60—75 lbs. of steam and makes about 70 revolutions per minute. This corresponds to a speed of $4\frac{2}{10}$ feet per second of the traveling rope, which therefore requires 14 minutes for a trip from anchorage to anchorage. The whole machinery, described above, is double; each set doing the work of

carrying wire for two cables. The driving power is derived from the same steam engine. During its passage from anchorage to anchorage, the working rope is supported on the towers and on each cradle by properly placed sheaves. In close connection with the working rope is the "traveling sheave."

This is a light wooden wheel 5 feet in diameter with a grooved rim of zinc, in which the wire hangs, while traveling across. It is secured to the traveler by an iron, bent in the shape of a goose-neck, so as to allow free passage of the rope over all supporting shears. An iron rod with a weight at the end, fastened to the hub of the wheel, keeps it in vertical position and prevents it being upset by the wind. Each working rope carries two traveling sheaves placed so, that one is at the New York anchorage, while the other is at the Brooklyn anchorage. When, for instance, the left sheave carries a wire from Brooklyn to New York, the right one moves empty in opposite direction. When the latter arrives at the Brooklyn anchorage, the driving wheel is stopped, a wire placed on the same, and the motion of the working rope is reversed.

Fig. 12



Wire Drum
Scale ¼" = 1 foot "
Clear width of drum = 15 feet
width over all = 21 feet

C. Iron hub.

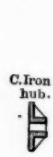
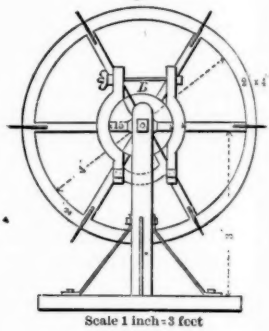
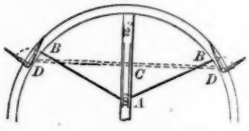


Fig. 13



Scale 1 inch = 3 feet

Fig. 14



Other appliances in the service of stretching wire are the wire drums. There are altogether 32 drums, 8 for each cable, the chief object of which is to serve as reservoirs of wire ready for being worked into strands. Each drum is 8′ 2″ in diameter, 15″ wide and can hold about 50,000 feet wire, which is enough for six to seven trips of the traveling sheave. It is provided with a brake by means of which the sag in the running wire can be regulated. It is necessary that the wire is wound tight on the drum, else the brake is ineffective. The wire ring therefore is placed first on a smaller wheel (Fig. 13), from which it is unwound on the large drum under considerable tension achieved by the brake *R* on the arms of the small drum. Small movable flanges (Fig. 14) admit putting the wire on and hold it afterwards in place.

2. *Cradles and Cradle Ropes.*— The cradles are necessary for various purposes. Their chief object is, as stated in the general description, to serve as platforms for men to stand on, who regulate the wires. They also are of value as intermediate supports of the traveling ropes and as a check for the lateral displacement of the wires by wind. Finally they serve as support to a few men, whose duty it is to throw the running wire on the other side of the strand.

There are altogether five double cradles, three in the river span and one in each land span. Owing to the fact, that the cables in the land span do not hang parallel to the axis of the bridge,

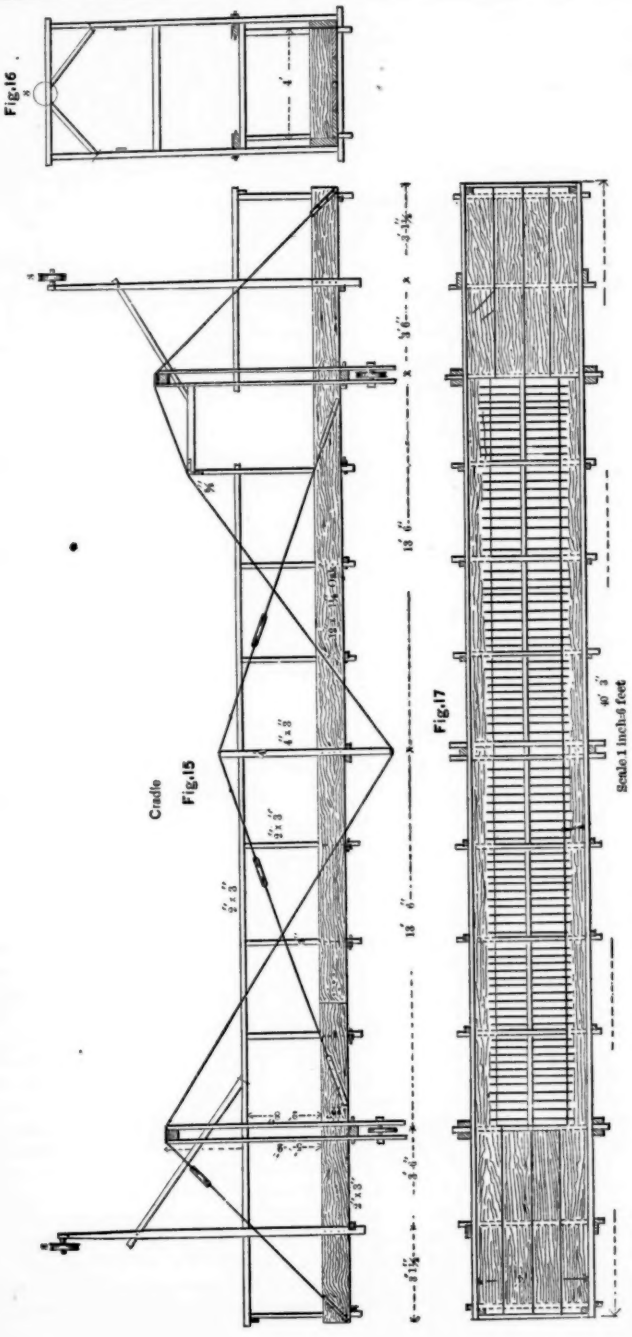


Fig.16
Cradle
Fig.15
Fig.17
Scale 1 inch=6 feet

but spread from anchorage towards tower, the single cradles have different lengths, those in the main span being 7 feet longer than the land cradles. On the opposite page one of the land cradles is illustrated in ground-plan, front and side elevation.

They are built throughout of oak and strengthened by light trussrods. The larger part of the platform consists of an iron grating for the free passage of the wind, to which they are dangerously exposed in their lofty position.

The *cradle cables* which support the cradles, consist of $2\frac{3}{8}$ inch crucible cast steel ropes with an ultimate strength of 180 tons each. One of the four ropes, which at the same time supports the footbridge is $2\frac{5}{8}$ inches in diameter, and able to sustain 240 tons. The following is the weight on each cradle rope:

Its own weight (9 lbs. per foot).....	14,580 lbs.
One half of three cradles.....	6,000 lbs.
Working rope.....	600 lbs.
About 6 men.....	960 lbs.
	22,140

It is suspended with a deflection of 73′3″ hence the largest tension in the rope is:

$$22140\frac{\sqrt{800^2\times 4\times 73.25^2}}{4.73.25}=61000\text{lbs.}=30\tfrac{1}{2}$$

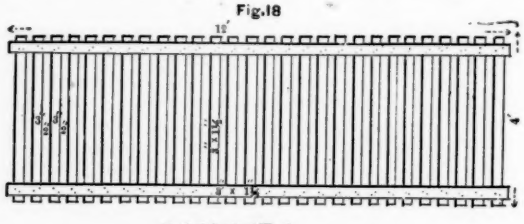
tons. This gives a margin of safety of 5.8 times.

On top of towers the cradle ropes rest on wooden blocks, and at each anchorage they are attached to separate anchor bars by means of wrought-iron sockets and screw stirrups, similar, only differently proportioned, to the connection shown in Fig. 21 and 22.

3. *The Footbridge.*—The footbridge has no direct service in cable making, but its indirect values are so great, that for the manufacture of large cables its erection may be deemed an absolute necessity. All the footbridges used by the late Mr. Roebling were built after the general plan of suspension bridges, namely, they consisted of a platform suspended to two cables. The writer of this, while working out the plans, suggested laying a platform directly on the cables, guided by the consideration, that a bridge parallel with the strand would enable the regulator to sight it at all points, that access to the cradles would be easy and that it would form a stiffer platform, which probably would suffer less under the violence of terrific gales, which rage almost weekly on the East River. Partially by these reasons, but especially for the accommodation of shipping, the Chief Engineer adopted this plan, and it was executed accordingly.

The floor of the footbridge (Fig. 18) is formed of oak slats $3''\times 1\frac{1}{2}$ inches, 4 ft. long, set two inches apart, and nailed to two longitudinal strips of equal size. It is made in sections of twelve feet length and rests on the main footbridge ropes, to which it is secured with small screw stirrups (see Fig. 19).

Fig. 18



Scale ½ inch = 1 Foot

Fig. 19

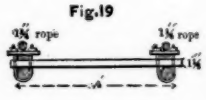
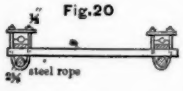


Fig. 20



The main footbridge cables consist of $2\frac{5}{8}''$ steel ropes, each having an ultimate strength of 240 tons. They are assisted by two auxiliary ropes of $1\frac{3}{4}$ and $1\frac{1}{4}$ inches in diameter; the heavier rope being on the side which also supports the cradles. The connection of these ropes with the floor is shown in Fig. 20.

The anchorage of the footbridge ropes is illustrated by Figs. 21 and 22.

The socket for fastening the rope in, is made of the toughest wrought iron, contrary to the usual cast sockets of this kind. This precaution was taken to exclude any danger of bursting, which, in a case like the present, would be followed by the greatest calamity.

The principal means for guarding the footbridge against gales, are two $1\frac{1}{4}$ inch ropes which, fastened at the towers, are,

Fig.21

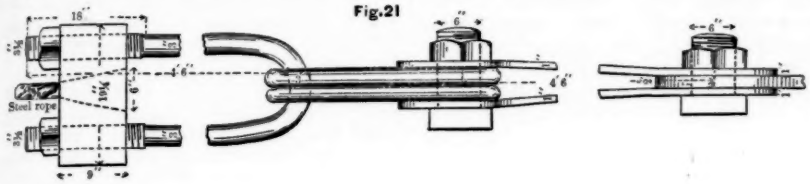
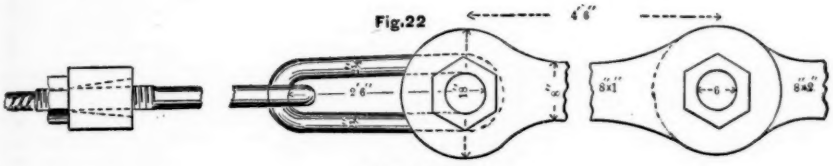


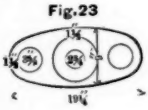
Fig.22



Scale ½ inch=1 foot

in shape of inverted parabolas, suspended from the footbridge by inclined suspenders (see Fig 1). These "storm ropes" are assisted by a number of storm stays, which reach from the tower out as far as the first cradles. In the land-spans the storm guys are anchored in the ground. This whole system of guying has proved very successful, as even in heavy gales the motion of the bridge is confined to a small lateral displacement.

Fig.23



The maximum load coming on one footbridge cable is 62123 lbs. creating a tension of 86 tons. This is resisted by the aggregate strength of the $2\frac{5}{8}$" and $1\frac{3}{4}$ inch rope amounting to 318 tons; hence the margin of safety is 3.7 times.

All the ropes used for the footbridge, cradles, etc., were manufactured in the works of John A. Roebling's Sons, Trenton, N.J.

The footbridge ropes were first suspended with a deflection of 64.4 feet and sunk to 74.2 feet after the load was on (a foot more than anticipated). This amounts to a stretch of 2.26 feet in the whole ropes, or to $\frac{1}{11800}$ of the length, per square inch of section, per ton of strain.

The erection of all these structures was a difficult and perilous task, considering that the lowest point is 200 feet above the water, and that about 100 craft of all kinds cross the line of the bridge every hour.

The first rope, taken over, was one of the travelers. Coiled on a reel, it was placed on a scow at the foot of the Brooklyn Tower. Its end was taken over the tower to the anchorage and temporarily fastened. Then the scow was towed across and the rope allowed to sink to the bottom of the river. The remaining part of the rope was taken from the reel and the end after passing over the tower, fastened to the drum of a steam engine. By observation it was ascertained, that most every day an accidental coincidence of circumstances keeps for 6–7 minutes the line under the bridge clear of vessels. Of such a chance advantage was taken, and in the forenoon of Aug. 14th, 1876, a cannon shot gave the signal for cutting the lashing on the Brooklyn dock and setting the engine in motion. Four minutes later the rope came out of the water and in six minutes hung clear over all masts and formed the first connecting link between the cities of New York and Brooklyn, destined never more to be broken. Simple as this operation was, it created considerable excitement and interest among the population, probably caused by a feeling of historical importance for the day, which practically should unite the two cities. Thousands of spectators lined the shores, who greeted with loud cheers the appearance of this little rope, as it rapidly ascended high up in the air. It seemed that all doubts, hitherto entertained as to the erection and subsequent safety of the bridge,

had vanished from that moment. The East River Bridge was considered an established fact.

The other half of the traveling rope was taken over in the same manner and then the ends of both were taken to the New York anchorage and spliced together, making the rope endless.

In stretching the second traveller, use was made of the one already in position. The new rope was lashed to it and pulled over by the engine. Afterwards these lashings were cut away, which was performed by men running out on the traveler in a boatswain's chair or a "buggy." The latter (Fig. 26) consists of a platform four feet square, suspended by four iron rods to a grooved six inch roller, which runs on the rope.

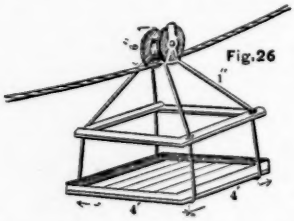


Fig.26

An auxiliary, so called carrier rope, of $1\frac{3}{4}$″ diameter was the next one stretched across. Its object was to carry the load of the footbridge and cradle ropes which were too heavy for the working rope. This carrier was hoisted up from out the water after the first method. The footbridge and cradle ropes were put in place in the following manner. Each rope was landed at the Brooklyn tower, its end hoisted up, passed around a sheave attached to the traveler, and lashed to a hemp rope which was connected with the drum of an engine at the New York anchorage.

As the rope moved on, it was supported by a "hanger" (see Figs. 24 and 25) every 50—60 feet, which consists of two hooks, for the rope to rest in, attached to a roller that runs on the carrier rope. These hangers were afterwards removed, leaving the rope suspended clear from the carrier.

After all ropes were in place, the cradles were hoisted on the towers and allowed to slide down to their proper positions, in which they are secured by heavy lashings.

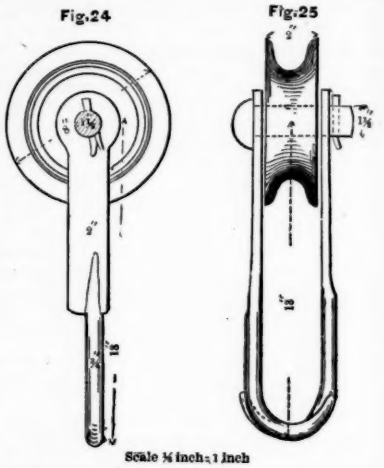


Fig.24 Fig.25

Scale ¼ inch=1 inch

Laying the footbridge floor was commenced simultaneously from both anchorages and the center of main span. It occupied but few days. Two light ropes, serving as handrails, lastly put in place, completed the footbridge, which since has been crossed by thousands of people.

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS.—The following papers are to be found in the Society's Transactions for June and July:

Connected-Arc Marine Boilers; a demonstration of the Principles of their Construction, by C. E. Emery.

On the simultaneous ignition of thousands of Mines, and the most advantageous grouping of Fuses, by J. H. Striedenger.

Improvement of Entrance to Galveston Harbor, by C. N. Howell.

The July number also contains an important discussion on the failure of the Ashtabula Bridge, by Messrs. A. P. Boller; E. Warren; E. S. Philbrick; C. Hilton; Thomas C. Clarke; Robert Briggs; C. Shaler Smith; J. Cooper.

THE INSTITUTION OF CIVIL ENGINEERS.—The session which ended in May has been one of more than usual activity both in respect of the ordinary operations of the society and of the interest aroused by certain changes proposed to be made in its constitution. The meetings began on November 14 last, when a Paper was read on "The Lighthouse System of Japan," which elicited much valuable information on the best form of structure calculated to withstand earthquake shocks. The discussion also afforded an opportunity for the former presentation of an interesting collection of specimens of the mineral products of Japan received from the Imperial Government. On this occasion Mr. Wooyeno, the Japanese Minister, was present, and personally respond-

ed to the vote of thanks passed to his Government. The next Paper, on "The Fracture of Railway Tires," sought to reduce to theory the difficult problem of the strains generated in tires by changes in the molecular constitution of the materials of which they are composed. Following upon this, and initiated by a Paper on the "Chalk Water System," was a discussion which lasted three evenings, the great question of metropolitan water supply being considered from widely different standpoints. The statutory annual general meeting of the members of the corporation was held on December 19. The presentation of the medals and premiums having been concluded, an important debate ensued upon the proposals of the Council for the establishment of another grade, intermediate between the existing members and the associates, and—much diversity of opinion existing—it was agreed to consult the whole body as to the title recommended by the Council for the new class. In the sequel, the difficulties of making any radical change in the case of an old-established corporation were fully realized, for though a majority supported the Council, a large and influential minority deprecated any change, and at a special general meeting, held subsequently, it was resolved that the object sought could be better attained by a literal interpretation of the existing bye-laws than by the enactment of new ones.

The ordinary meetings were resumed after the Christmas recess by the reading of a Paper "On the Repairs and Renewals of Locomotives." This was followed by one "On the Combustion of Refuse Vegetable Substances for raising Steam," a subject of considerable though indirect interest to the general public, for in it is involved the question of the cost of importing flour from the great corn-growing countries of south-eastern Europe, where coal for the threshing engines is not to be had, and wood is getting dearer and dearer. Next ensued a discussion upon "The Sewage Question," which occupied five evenings, thus testifying to the interest felt in the subject and to the eagerness with which the solution of its difficulties would be hailed. Attention was then directed to "Transmission of Power," in which various systems and agents were alluded to. A Paper "On the River Thames" could not fail to excite attention at the present time, though the discussion rather turned upon the "regulation" of the river than upon the causes of late floods. To this succeeded an account of a "Deep Boring for Coal at Scarle in Lincolnshire," and a Paper "On Street Tramways," and these gave place to one "On the History of the Modern Development of Water Pressure Machinery," in which was traced the growth of the present system of applying hydraulic power to numberless purposes formerly supposed only capable of being met by steam. A rather significant account of an improvement in detail affecting the cost of making gunpowder charcoal completed the ordinary business of the session, which was closed, in accordance with a time-honored custom, by a conversazione given by the president this year at the South Kensington Museum on Thursday evening. The papers noticed will, with matter from various other sources, form material for four octavo volumes of "Proceedings," two of which have already been issued, and the others will follow in August and November respectively. This account of the sessional work of one of the Institutions may be fitly closed by a few particulars respecting its position. The Institution of Civil Engineers was established in 1818 "for the general advancement of mechanical science, particularly civil engineering," and now numbers upwards of 3,104 members of all classes.

It is the custodian of trust funds amounting to more than £14,000, the interest upon which is devoted to premiums for meritorious original communications made to the society. It also possesses funded property available for the general benefit of the society of £22,500, has an annual income of between £10,000 and £11,000, and possesses an unique and unrivalled engineering library of 13,000 volumes.

IRON AND STEEL NOTES.

Homogeneous Iron.—A general meeting of the members of the Institution of Mechanical Engineers took place on Thursday, the 31st May, at the Institution of Civil Engineers. A discussion took place on Mr. Kirk's paper "On Homogeneous Iron and the degree of Homogeneity to be expected in Iron produced by various systems of Puddling and subsequent Working." Mr. Kirk made some preliminary remarks, in the course of which he stated that the opinion expressed in his paper that the Danks furnace did not give much promise of homogeneity in its products, because of the difficulty of expelling the large quantity of thick cinder usually present, had recently been to some extent modified by the results of some experiments. He thought that rotary furnaces were adapted to the production of a steely iron, though he had found that the two ends of a bar produced from iron puddled in a Danks furnace had given analysis which showed the percentage of sulphur to vary between about 0.3 and 0.23 per cent. He also remarked on the general superiority of crystalline iron, saying that most of the bar iron which will stand bending and doubling cold is of a crystalline character, and that usually this class combines to a greater degree the properties of toughness and elasticity than that which breaks with a fibrous fracture. The latter characteristic he attributed to the presence of cinder, and suggested that cinder having practically no tenacity, the strength of iron in which it existed must be reduced in proportion to the quantity contained. Dr. Siemens agreed generally with Mr. Kirk that a crystalline structure was not antagonistic to toughness, and that it generally indicated a freedom from cinder. Cinder, he said, could be of no service in the composition of iron. He then referred to the papers which he read in 1868 before the British Association dealing with the question of boiling in the puddling furnace, and containing his suggestion that as much weight of puddled bar should be obtained as there was of iron put into the furnace. The iron is not

so much oxidized in the rotating gas puddling furnace as in the ordinary fixed furnace, the flame being more of a neutral character. Reference was then made to the very successful working of the gas furnaces at Messrs. Nettlefolds, Wellington Works, the results of their operation throughout eighteen months being of a most satisfactory character; 12,384 tons of bars had been made, and the loss of iron in the furnaces was only 3.8 per cent. Dr. Siemens had more recently turned his attention particularly to the production of wrought iron from the ores. It was, he said, a difficult problem, but he believed he might consider it now practically, though not commercially, solved. He was using Northamptonshire ores, containing 30 per cent. of metallic iron and about 2.5 per cent. of phosphoric acid, equal to about 4 per cent. of the iron in the ore, though only about 0.1 per cent. of phosphorus was found in the iron produced. The iron was produced by means of a considerable admixture of silex in the rotary gas furnace, and the explanation of the absence of phosphorus was referred to the neutral character of the flame, which admitted of its being carried off with the slag, instead of being combined with the silicon, and thereby with the metallic iron, as was the case in the blast furnace. Mr. Bell could not see in what way the presence of cinder influenced the strength of iron, as, assuming the iron and cinder to be in layers, the iron would still be in a condition to exercise its resisting properties, and cinder could not render iron brittle. He could not agree with Dr. Siemens with reference to the possibility of even an approach to a neutral flame, as carbonic oxide was introduced into the furnace with the gas, and a very small quantity of carbonic oxide would oxidise a considerable weight of cast iron in an ordinary furnace. He was inclined to think that the good results obtained from rotary furnaces was due mainly to the mechanical regularity and quickness of the apparatus, and that the removal of the phosphorus described by Dr. Siemens was not at all due to the nature of the flame, but that it was entirely a question of temperature. In the Siemens furnace the temperature was kept below that at which phosphorus combines with iron, and as iron oxidises so much more readily than phosphorus or sulphur, the latter in the Siemens furnace have no opportunity of combination with the former. Hence the freedom from these described by Dr. Siemens. Mr. Crampton spoke particularly with reference to the good results obtained by Price's furnaces, over 1000 tons of iron having recently been obtained, which was within one per cent. of the quantity put into the furnace. Mr. Crampton was disposed to attribute most of this success to the quickness with which the operations were conducted, and to the fact that the fuel was passed into the furnaces in an incandescent condition, and that no cold air was at any time admitted. In many cases the excess of yield was due to the fettling. Mr. Jeremiah Head thought that the meaning attached to the word homogeneous, as now so often used with reference to the properties of iron and steel, should be defined, and that it would be regretted if any misapplication should lead to such words having different meanings in different languages, though originally they had one common derivative. He thought that the experiments of Tyndall on the adhesion of surface plates suggested that the presence of cinder must certainly tend to the loss of strength in iron. One surface plate supported the lower one by attraction of cohesion; the attraction of magnetism decreased as the square of the distance between the objects, and from these facts it might be argued that the presence of any impurity which kept the particles of iron asunder reduced the strength of the whole, and cinder acted in this way. He referred to the fact that the difference between the strength of rolled iron with and against the grain was most apparent in long narrow plates and bars, and that the Lowmoor Company are now putting down rolls 11 ft. to 12 ft. long, so that the rolling action may act orthogonally, and the plates produced may be equal in strength in all directions.

RAILWAY NOTES.

NEW ZEALAND RAILWAYS.—The New Zealand Government has ordered surveys to be made for a railway between the Thames and the Waikato. The new line is expected to prove one of the most successful railways in New Zealand.

CANADIAN PACIFIC RAILWAY.—Mr. Sanford Fleming has reported on the surveys and preliminary operations carried on in connection with the Canadian Pacific Railway. Mr. Fleming states that the surveys, commenced in 1871, now extend from the valley of the Ottawa, west of the capital, to that portion of the Pacific cost lying between Alaska on the north and the Straits of San Juan de Fuca on the south. The cost of the surveys to the close of December, 1876, had been $3,136,165. From Burrard Inlet to Montreal will be 633 miles less than the distance from San Francisco to New York. It is estimated that the Canadian route will bring New York, Boston, and Portland from 300 miles to 500 miles nearer the Pacific coast at Burrard Inlet than these cities now are, with San Francisco as the terminal point of the Pacific Railroad. The distance from England to China will be more than 1000 miles less by the Canadian than by the American line. Ten routes for the Canadian Pacific line have been opened for consideration; they terminate on the coast of the main land at seven distinct harbors, and they all converge to Yellow Head Pass. The line has been located with sufficient accuracy to admit of the construction of an overland telegraph.

ENGINEERING STRUCTURES.

ACTIVE preparations are going on for the immediate commencement of the long-projected work of draining the Zuyder Zee. A dam of 40 kilometers—24 miles 1504 yards —long, and 50 metres broad at its base, is to be

carried across the gulf, but up to a height of ¼ meter above the ordinary level of high tide. Upon this pumping machines of 10,000-horse power will be erected, capable of pumping up from the enclosed sea and discharging on the outside of the dam 6,500,000 cubic meters of water daily. Taking the average depth of the water at 4½ meters, it is estimated that the work of pumping will be completed in about sixteen years from its commencement. The total cost of reclamation is set down at 335,000,000f., but, huge as this sum is, the undertaking is confidently looked upon as likely to prove a most remunerative speculation. The success of the scheme will add to the kingdom a new province 195,300 hectares—or nearly 500,000 acres—in extent. Judging from previous experiences in connection with the Haarlem Sea, it is reckoned that at least 176,000 hectares of the land thus won will be applicable to agricultural purposes, which, at an average value of 4000f. only per hectare, will richly repay the enterprise and treasure lavished on the gigantic undertaking.—*The Farmer.*

PASSAGE OF THE SUEZ CANAL BY DEEP DRAUGHT IRONCLADS.—Mr. Latimer Clarke, C E., read a paper on the 11th inst. before the Royal United Service Institution, "On the Employment of Clark and Stanfield's Floating Docks at Naval Stations and the means they afford of transporting heavy Ironclads through the Suez Canal." Mr. Donald Currie occupied the chair. Mr. Scott Russell, Captain J.C.R. Colomb, R.M.A., Mr. Rennie, and other well-known gentlemen took part in the discussion which followed, and which was prolonged to an unusually late hour, the subject being one of both pressing importance and great interest, the paper containing many startling facts which it would be well to calmly consider in time of peace to prevent ourselves being found unprepared in time of war. The real economy of providing in time, and at our leisure, the necessary means of repair to our complicated fighting vessels at our distant vulnerable points, was forcibly pointed out, and indeed there can be no doubt that few are really aware of the helpless state into which our naval forces at long distances from home could be speedily thrown for want of a few coaling stations, cheap and portable docks, and the necessary telegraphic connections.

The transport of our heavy ironclads through the Suez Canal has always been considered a sheer impossibility, but these floating docks, provided with air cushion blocking, acting somewhat similarly to the well-known Russian air camels, appear to efficiently overcome the difficulties presented by the problem; it is very obvious that in time of war the practical value of our most powerful ships would by these means be enormously increased.

ORDNANCE AND NAVAL.

A NEW MILITARY INVENTION.—The German General Berden has devised a new distance-calculator, or, in military language, "range-finder." When closed up ready for moving it looks like a sort of primitive chariot, mounted on two large light wheels and drawn by one horse. Two men ride upon the comfortable seat, the driver the operator, and for the latter, so simple is the instrument that no special training is necessary, however desirable it may be that he should also understand the principle of trigonometrical science. When the range of any point is to be taken, before a battery begins to play, for instance, or a regiment to fire, the horse is slipped out of the shafts, and the body of the "chariot" then turned completely over on its axle. The frame on which is built the seat, then serves as a firm and steady support for the instrument. The body of the vehicle is revealed as a box or case one metre wide, nearly two long, and about a foot deep; and when its two opposite ends are thrown open, the instrument is ready for use. This consists, loosely described, of two parallel telescopes, about 1½ metres long, and very powerful, affixed to a frame which swings on a common pivot. The sight ends are just one metre apart, and this is, of course, the base line of the calculation. One of these telescopes, the one at the right hand, is movable only with the frame, of which both form a part. This is first sighted on the object, and the frame is made fast, the first step being thus completed. The other telescope is adjustable further, on a pivot of its own, and by means of a small wheel is turned to the right angle of convergence with its companion—that is to say, until it too covers the object. Now, having the base line and the angle of convergence, any surveyor could, of course, calculate the distance. But in General Berdan's instrument the wheel which adjusts the second telescope is marked off into meters, centimetres, and milometres, is covered by a little hand or pointer, and when the object is brought within the focus the pointer indicates exactly the distance. This is absolutely all there is of it. In two minutes the instrument can be unlimbered, put into readiness, and a distance found, less time therefore than a gun or a battery is made ready. Then the two ends are closed, the body swung around into its place, the horse put into the shafts, the driver and operator mount the box, and away they dash to some other point.

BOOK NOTICES.

BRITISH INDUSTRIES. SEA AND SALMON FISHERIES. By E. W. H. HOLDSWORTH and ARCHIBALD YOUNG. London: Edward Stanford. For sale by D. Van Nostrand. Price $2.25.

The two authors treat respectively of Sea Fishery and Salmon Fishery.

The book is descriptive only of methods pursued in Great Britain. The description of apparatus is made clear by good diagrams.

The contents are: Sea Fisheries; English Fisheries; Scotch Fisheries; Manx Fisheries; Irish Fisheries; Salmon Fisheries.

The growing interest in this latter subject, under the stimulus of the Commissioners of State Fisheries in this country, will doubtless create a demand for this little book.

COMMON-SENSE FOR GAS USERS. A catechism of Gas-Lighting. By ROBERT WILSON, C.E. London: For sale by D. Van Nostrand. Price $1.00.

This book is designed for consumers of gas rather than for Gas Engineers.

It contains much good advice regarding selection and testing of burners, care of meters, etc., put in the plainest possible style. The illustrations are numerous and good.

An appendix treats, in a brief and satisfactory way, of the utility of gas engines.

A GENERAL CLASSIFICATION OF RAILWAY RIGHTS, REALITIES, AND PERSONALTIES. By GEORGE T. BALCH, C.E. New York: J. J. Little & Co.

This work is designated to facilitate the labor of taking an inventory of railway property. It is illustrated by the example of the Receiver's Inventory of the property of the Erie Railway Company, as prepared for the Supreme Court of the State of New York. Not a widely useful book, but invaluable in certain cases.

HANDBOOK OF NATURAL PHILOSOPHY: MECHANICS. By DIONYSIUS LARDNER, D.C.L. New edition. Edited by BENJAMIN LOEWY, F.R.A.S. For sale by D. Van Nostrand. Price $3.00.

The original work of Dr. Lardner has been used so far as the elementary exposition of principles is concerned, but the whole has been revised by the new editor, so as to bring it in accord with modern scientific terminology.

The present issue is the first of five proposed volumes, to treat respectively of Mechanics; Hydrostatics; Pneumatics; Heat; Optics; Electricity; Magnetism, and Acoustics.

The work has always deservedly enjoyed a high reputation for clearness of exposition of the most abstract principles of physics.

VAN NOSTRAND'S SCIENCE SERIES, NO. 30. THE MAGNETISM OF IRON VESSELS. By PROF. FAIRMAN ROGERS. New York: D. Van Nostrand. Price 50 cts.

The readers of the Magazine have already been made familiar with this able work, through the pages of the numbers for May and June last.

It is rare that so much science is found in so compact a form as is now offered in this little treatise.

It is designed for the use of all engaged in navigation, especially to yachtsmen and scientific travellers, as a simple introduction to the subject, and a guide in such observations as they may feel disposed to undertake.

AN ELEMENTARY TREATISE ON THE INTEGRAL CALCULUS. By BENJAMIN WILLIAMSON, M. A. Second Edition, Revised and Enlarged. London: For sale by D. Van Nostrand. Price $5.25.

This work, the first edition of which we noticed last year, appears to have satisfied a wide-spread want on this side of the Atlantic. An examination of the table of contents will reveal the fact that applications of the Calculus occupy considerable space, nearly half the book.

I. Elementary Forms of Integration. II. Integration of Rational Fractions. III. Integration by Successive Reduction. IV. Integration by Rationalization. V. Miscellaneous Examples. VI. Definite Integrals. VII. Areas of Plane Curves. VIII. Lengths of Curves. IX. Volumes and Surfaces of Solids. X. Moments of Inertia. XI. On Mean Value and Probability.

WATER SUPPLY ENGINEERING. By J. T. FANNING, C. E. New York: D. Van Nostrand. Price $6.00.

This is an elaborate treatise relating to the hydrology, hydrodynamics, and practical construction of public water supplies.

Our author says in his preface, "This work is intended more especially for those who have already had a task assigned them, and who as commissioners, engineers, or assistants, are to proceed at once upon their reconnoissance and surveys, and the preparation of plans for a public water supply."

A perusal of the contents reveals a systematic, progressive method of treatment of the general subject, that leads the engineer confidently forward from his reconnoissance in the field to the delivery of water through the arterial net-work of distribution pipes and fire hydrants, amid the households and warehouses of a town or city.

The book is divided into three sections, and treats upon—1st. The Collection and Storage of Water, 160 pages; 2d. The Flow of Water through Sluices, Pipes, and Channels, 172 pages; 3d. The Practical Construction of Water Works, 259 pages.

An introductory chapter discusses the physiological and financial influences of a liberal distribution of wholesome water throughout a city, and points out their values, especially those incidental values that do not appear upon the balance-sheet of the City Treasurer, but which are invariably of far greater importance respecting the welfare of the city than the construction, maintenance and income accounts.

The introductory chapter is followed by statistics and tables of water supplied and consumed in various American and foreign cities, and of the ratios of consumption during the different seasons, days of the week, and hours of the day, and of the reserve and conduit capacity necessary to provide water for the uses of the fire department.

The useful statistics here collated, and illustrative diagrams, will prove of exceeding value to engineers and commissioners who have to estimate quantities of water that must be provided in new water supplies, since the quantity of water required is a controlling element in the preliminary investigations, in the proportion of conduits and distribution pipes, and in the capacities of reservoirs, filters, and pumping machinery.

The hydrology of water supplies is discussed in the several chapters upon *Rainfall, Flow of Streams, Storage and Evaporation of Water, Supplying Capacity of Water-Sheds*, and upon the supplies from *Springs and Wells*. These chapters relate more especially to the hydrology of the United States.

The rainfalls are first classed into Western or Pacific coast, Central or Mississippi valley, and Easter or Atlantic coast systems, and the influences controlling each examined. The rainfalls of the principal river basins of each main system are then independently examined, and the variations in precipitation from source to *debouche* are shown in a table convenient for reference. There are tables and diagrams of grouped rainfall statistics, and discussions of the monthly and secular variations in precipitation, and of the local and physical meteorological influences which are not to be neglected in an estimate of the rainfall that may be expected, and that may be made available in any given locality.

The vast current that sweeps across the tropical Atlantic, and impinges upon our Atlantic coast, and from which branches a broad stream that passes up through the Caribbean Sea and circuits through the Gulf of Mexico, yields a flood of warm vapor that is wafted by prevailing winds up the great trough between the Alleghany and Rocky Mountain ridges and over the Atlantic slope of the Appalachian range. An equally vast current sweeps up past the coast of China and the Aleutian islands and impinges upon our Pacific slope, and the prevailing westerly winds waft its floods of accompanying vapors up the westerly slopes of the coast range, the Sierras and the Rocky Mountains, and a portion of this latter vapor, after the passage of the great ridge, is gathered into the storms that sweep down across our northwestern States and the lake region.

The influences of the great ridges upon the flow of vapor from the two great ocean currents is marked, and a study of them leads easily to localization of dependent rain systems of uniform characteristics. And more detailed study leads easily to the special characteristic of precipitation in each of the great river basins. The statistics in the chapter upon rainfall are classified upon the system above indicated, and include reports from stations in nearly all sections of the State and territories, so that the reader will be able to determine from the data given, with close approximation, the mean rainfall in any of the principal river valleys, and by comparison, also, in any lesser valley of the tributaries of the main streams.

The chapter closes with descriptions of some of the great storms that have moved over the Atlantic slope and New England, and with instruction for the selection of pluviometers, and their management, for reliable gaugings of rainfalls.

The elaborate discussion of rainfall forms a fitting introduction to the succeeding chapter upon the *Flow of Streams*. The flow is dependent not only upon the periodic fluctuations in precipitation, but largely upon evaporation and other influences. The causes that produce steady or flashy streams are duly considered, and the periodic divisions of natural flow are classified, and the monthly ratios of flow are developed from statistics of actual measurements. Several tables and formulas to facilitate estimates of minimum, mean and flood flows of streams, are presented.

Next follows a brief chapter upon the artificial *Storage of Water*, and the losses of water by *evaporation, percolation,* and other means, incident to storage. This chapter gives numerous valuable statistics and tables relating to evaporation from water, earth, and vegetation, and their practical effect upon storage.

The succeeding chapter upon the *Supplying Capacity of Water Sheds*, is a practical application of the deductions of the three previous chapters just alluded to. This discussion will prove invaluable to all those who are interested in the gathering of water for power, domestic use, or for irrigation, or who have to guard against the disasters of floods, and sufferings incident to severe drought.

The next two chapters treat upon the *Impurities of Water*, and upon the qualities of *Well, Spring, Lake, and River Supplies.* Our author, recognizing the vital influence of one of the three essentials to human life, *air, water*, and *food*, has examined minutely the composition, solutions in, and properties of water, and the physiological effects of its impurities. Its usual mineral and organic impurities, and their sources, whether belonging to the surface, sub-surface, atmospheric, terrestrial, aquatic, animate or vegetal classes, are duly pointed out, so that their presence or absence may usually be predicted without the aid of qualitative analysis. The qualities and deadly effects of agricultural, manufacturing, and sewage impurities, and the effects of impure ice in drinking water, from which much sickness results in cities, are vividly pictured and an energetic protest entered against the admission of such filthy dregs into the drinking waters of towns, and by it into the human circulation. Some valuable hints are given for the selection, test of quality, and preservation of domestic water supplies. A great deal of the information here condensed is invaluable in every household, for its own protection, for it is a lamentable fact that in many towns the subtile poisons of impure water are flowing day by day unsuspected into the life-blood and weakening, especially, the systems of the growing children. A knowledge of these impurities and their sources will lead to their avoidance.

The opening chapter of the second section is devoted to the consideration of the *Weight, Pressure, and Motion of Water*, more especially to those molecular properties that give to water its mobility, its possibility of equal pressure in all directions, and the possibility of the instantaneous conversion of the pressure among the particles into motion of the particles. It considers also the theoretical effects of pressure due to the combined weight of the particles and the theories of the resultant pressures upon plane and upon curvilinear surfaces, and gives useful formulas and tables to facilitate calculations of pressures in reservoirs, chambers, pipes, and upon sluices, or coffer-dams. This chapter is a concise presentation of those philosophical principles applicable to the pressure of water with which the student in hydrodynamics must needs be familiar before he attempts the design of hydraulic structures or machines, and which will make easily compre-

hensible the theory of flow of water as discussed in the next five chapters relating respectively to the *Flow of Water through Orifices, Flow of Water through Ajutages, Flow of Water through Pipes under Pressure, Flow of Water in Open Channels,* and to *Measuring Weirs and Weir Gauging.*

The second section of the book exhibits by plain language and pictorial illustration the methods by which the constant force of gravity is applied to the determination of the volume of flow of water through orifice, ajutage, weir, pipe, and channel. Gravity acts upon the individual particles as it acts upon a solid, but when the particles fall through an orifice, or through an ajutage or a weir, the volume is modified by the contraction of the jet at the issue, and when they fall, or, as it is usually expressed, flow through a pipe or channel, the constantly accelerating force of gravity is modified by the reactionary resistances from the pipe wall, or channel sides and bottom. In the cases of pipes and channels, the effect of the accelerating force is directly as the ratio of inclination, and the retarding force is directly as the area of reactional surface of pipe or channel, the square of velocity of flow, and inversely as the sectional area of stream, modified by a coefficient. The flow becomes uniform when the accelerating and retarding forces balance each other, and the flow under this condition becomes subject to closely approximate measurement. From the conditions as above stated, it is seen that an equation may be formed for any given case of uniform flow, in which the velocity and the coefficient will be the only unknown quantities. Now if a great variety of experimental measurements of flow in pipes, for instance, with varied velocities and varied sectional areas be collated, and their coefficients determined and systematized by plotting, and thus a full table of coefficients applicable to all common cases be deduced, then a simple equation, having but one unknown quantity, may be constructed for each given case, and that unknown quantity—the velocity, can be easily computed. Tables of coefficients are presented in these chapters, with simple formulas to which they are applicable, and adapted to the several classes of orificed jets, and to streams, and adapted to compute the velocity, the head to which the velocity is due, diameter of jet or pipes, and volume of flow. It is here shown that the series of coefficients applicable to smooth pipes is not applicable to masonry conduits, or to open channels, neither are the plotted curves of the several series parallel. Hence the confusion, when the attempt has been made to make some one of the old standard formulas for flow universally applicable. In the discussion upon the flow of streams, the most approved forms of hydrometers are illustrated, and their methods of use described.

The third section of the book, which includes about one-half the entire work, is devoted entirely to practical engineering construction, and is very fully illustrated with plates and cuts of suggestive type-forms of complete structures, and details. The first practical subjects taken for descriptive discussion are *Reservoir Embankments and Chambers, and Canal Banks.* The author seeking for underlying principles, considers not merely the metallic and cohesive natures of the earths of which he would build an embankment, but also their interstices and the combinations of earth grains that will reduce the voids to the smallest microscopic proportions, and shows clearly why one bank of earth is impervious and stable, and another is constantly liable to be swept away. The proportion of waste-ways, the safety-valves of embankments are more fully discussed than they have heretofore been in any publication, and formulas and tables of their dimensions are given.

The construction of *Waste Weirs* and *Dams* of masonry and timber crib-work is fully exemplified by varied illustration and description. Then follows a chapter devoted to the theoretical proportions and practical construction of masonry partitions and retaining walls to sustain earth or water, and of wharf and pier revetments. The theoretical profiles of retaining walls have been favorable subjects of analysis in technical treatises, and in the class-rooms of technical schools, and favorite subjects for the application of mathematical gymnastics, but the instructive judgments of intelligent, practical mechanics often rebel against the acceptance of the theoretical deductions, because erroneous or unpractical bases have been assumed. The present discussion shows that theory and good mechanical practice are really in harmony, as developed by one familiar with both theory and practical construction.

The next three chapters are devoted to the proportions, construction, and laying of conduits of masonry and mains and distribution pipes of metal, and the valves, hydrants and appendages of distribution systems. Many valuable suggestions evolved from the author's extended practical experience are herein contained, and accompanied by tables that will greatly facilitate the preparation of plans of pipe systems.

The next chapter is devoted to the *Clarification of Water.* The various sediments and impurities which the water brings in suspension or in volution are duly considered, and then the processes of treatment by infiltration, precipitation and filtration are described. The methods of construction of filter-beds and basins are illustrated and described, including instructions for their management and maintenance.

A chapter is devoted to the approved methods of pumping water, the various types of pumps, and prime movers, and to the discussion of the efficiencies of the various parts, such as turbine and boiler, steam cylinders condenser, pump-cylinder, air-vessel, etc., that form the combination of a pumping engine.

The concluding chapter is a brief discussion of the several *Systems of Water Supply,* and includes a review of the methods of gathering and delivering water, the choice of water, and systems of pumping with reservoir reserve and pressure, and with direct pressure, and by either steam or hydraulic power. An appendix is added, giving numerous tables,

equivalents, and formulas, of value to hydraulic and mechanical engineers.

The work covers 650 pages, octavo, and contains 32 full-page plates, 190 illustrations, and 130 tables. It has been carefully indexed, both alphabetically and by subject, to facilitate convenient reference to the details of subjects treated upon.

While professedly intended as a manual for water supply engineers, and water commissioners, it is really the most thorough, complete, and systematic elucidation of the elementary principles of hydrology and hydrodynamics, with their application to practical hydraulic constructions and water supply systems that has yet appeared in the English language. The writer's long experience and reputation as a successful designer and constructor of hydraulic works and machinery, should give weight to what he has to say on these subjects, and his exhaustive treatment of his subject will undoubtedly give to this book the position of a standard treatise on hydraulics.

The publisher has not spared expense in the production of the work, and has given it that mechanical and artistic perfection which its merits deserve.

A MANUAL OF THE MECHANICS OF ENGINEERING AND OF THE CONSTRUCTION OF MACHINES, WITH AN INTRODUCTION TO THE CALCULUS. By JULIUS WEISBACH, Ph. D. Vol. 1. Theoretical Mechanics. Translated from the fourth augmented and improved German edition. By Eckley B. Coxe, M.A. London: Trubner & Co.; 1877. New York: For sale by D. Van Nostrand. Price $10.00.

When a book has reached four editions it may be taken for granted that whatever its merits or demerits may be, it supplies a want, and is appreciated by those for whose use it is intended. The first edition of the work before us was published in March, 1846; the second in May, 1850; the third in July, 1856; and the fourth in May, 1863. The present translation seems to have been finished in December, 1876, in America, and was published in this country in the present year. To a certain class of engineers Weisbach's "Mechanics" has been well known for years; but the rising generation have little acquaintance with it. We venture to hope that this will not much longer be the case, for it is indisputable that the work is one of the most able and complete ever written on the mechanics of engineering. To the non-mathematical reader, however, its appearance is singularly repulsive, as its pages fairly bristle with formulæ. In this it has not a little in common with Rankine's works. On examination, however, it will be found while it is no doubt "tough reading" in some respects, the style is so good and the drawings and diagrams so excellent, that a great deal of valuable information can be obtained from it by anyone possessed of so much mathematical knowledge as will enable him to read a formula. The method of the author is very different from that of Rankine. Instead of contenting himself with giving a formula or a rule, Dr. Weisbach has, in most instances, appended an example to show how the formula is to be applied. Nothing can be devised which is more likely to assist the student than this. It throws light on difficult points; clears equations of letters and replaces them by figures which always appeal more powerfully to the practical mind; and in not a few instances reduces apparently very difficult problems to a rule of three sum. The method of treatment adopted by our author is, however, essentially mathematical. He saw plainly that a multitude of questions have to be dealt with by engineers which can only be handled readily by the aid of the higher mathematics; but he also discovered that the utility of his first edition was seriously impaired by the fact that large numbers of engineer students knew nothing of the calculus. To get over this difficulty he added to the second edition an introduction to the calculus, which in lucidity and comprehensive grasp of the subject, has never been surpassed. He thus, in a measure, supplies the key by which the storehouse of knowledge contained in his volumes can be thrown open. The book is so rendered complete in itself, and can be read without extraneous aid by any one who can master the first chapters either with or without the assistance of a teacher.

The book is of such great magnitude—containing, as it does, no fewer than 1,112 pages, and 902 woodcuts—that we may be excused from doing more than giving a general idea of the subjects with which it deals. We have first the introduction to the calculus, to which we have just referred. Next comes Section I., which deals with phoronomics, or the purely mathematical theory of motion. The first chapter treats of simple motion, and the second of compound motion. Section II. is devoted to mechanics, or the physical science of motion in general. This also is divided into two chapters. The first deals with the fundamental principles and laws of mechanics, and the second with the mechanics of a material point. Section III. is devoted to the consideration of the statics of rigid bodies. It contains five chapters on the general principles of the statics of rigid bodies, the theory of the centre of gravity, the equilibrium of bodies rigidly fastened and supported, equilibrium in funicular machines, and the resistance of friction and rigidity of cordage. Section IV. deals with the application of statics to the elasticity and strength of bodies. In it are five chapters on compression and shearing, elasticity and strength of flexure or bending, the action of shearing elasticity on the bending and twisting of bodies, on the proof strength of long columns, and on combined elasticity and strength. Section V., on the dynamics of rigid bodies, contains four chapters on the moment of inertia, centrifugal force, the action of gravity, and the theory of impact. Section VI., on the statics of fluids, has four chapters on the eqilibrium and pressure of fluids, the equilibrium of water with other bodies, the molecular action of water, and the equilibrium and pressure of the air. Section VII., on the dynamics of fluids, has nine chapters on the efflux of water from vessels, the contraction of the vein when issuing from an orifice in a thin

plate, the flow of water through pipes, the resistance caused by sudden enlargement and contraction in the pipes, the efflux of water under variable pressure, the efflux of air and other fluids, the motion of water in canals, hydrometry, and the impulse and resistance of fluids. In an appendix, we have the theory of oscillation fully considered.

The preceding statement will serve to give an idea of the enormous range of the book, and it will show that scarcely a road which the engineer can travel has been left unprovided with finger-posts by our author.

Before concluding, we must raise our voice to protest against the unwieldy form in which the book has been issued by the publishers. It is in dimensions, just such another volume as Clark's "Rules and Tables," to the size of which we have already taken exception. It weighs no less than 3¼ lbs., and is far too thick to be read with comfort. Such voluminous works would be much more easily used if they were issued in two parts. If this be deemed objectionable, then a larger page should be used, which would give a thinner and handier book. For the rest—the type, printing, and paper, are all good, but we should like to know who is responsible for the grave defect that in numerous instances commas have been used instead of decimal points.—*The Engineer.*

MISCELLANEOUS.

Novelty in Bridge Construction.—A most remarkable bridge, on a new principle, was opened on the Tyne some time ago. It is connected with the Scotswood and Wylam Railway, and crosses the Tyne near Wylam. It is on the bowstring girder principle. The girders are made from wrought iron, and the roadway is suspended from these girders. The bridge has a very light appearance, and there was a general impression that it would collapse when tested, but the severe tests applied to it have shown that the principle of the bridge is good, and also that the material and workmanship are of the highest class. Enormous weights, in the shape of engines, tenders, &c., were placed upon the bridge in various positions, and the deflection was so small that the utmost confidence is now felt in the safety of the structure. These experiments were conducted by the Government Inspector, Mr. Alfred Hannen, Mr. Haswell, and others. The final experiment was as follows—Three engines, coupled, were started from the south end of the bridge, and the same number from the north end, and they were suddenly stopped at the center, and even with this test the deflection and vibration were scarcely visible.

The Turkish Navy.—In addition to the particulars given in a previous issue (No. 211) the following information with regard to the present condition of the Turkish Navy will be interesting at this time. There are at present fifteen ironclads ready for active service, two nearly completed and two building. In addition to these vessels, Hobart Pasha has under his command fourteen monitors for coast and river service, which will be able to do good work in hindering the Russian army from crossing the Danube. Of the fifteen ironclads, seven are frigates and eight corvettes; they are all armed with Armstrong breechloaders. The particulars of the fleet are as follows:—Frigates: Massoudieh, 14 guns, 7200 h.p.; Musreetieh, 12 guns, 800 h.p.; Azizieh, 16 guns, 3050 h.p.; Osmanieh, 16 guns, 3050 h.p.; Orkanieh, 16 guns, 3050 h.p.; Mah-mudieh, 16 guns, 3050 h.p.; Ashar-i-Tefzik, 8 guns, 3568 h.p. Corvettes: Mahaden-Hair, 4 guns, 3250 h.p.; Feth-i-Bulend, 4 guns, 3250 h.p.; Idaladieh, 5 guns, 300 h.p.; Aoni-Illah, 4 guns, 400 h.p; Aschar-Chef-kit, 5 guns, 300 h.p.; Neduh-im-Shefket, 5 guns, 350 h.p; Lufi-i-Dchelik, 5 guns, 200 h.p.; Hifs-e-Rhaman, 5 guns, 200 h.p. The crew of each of the frigates has recently been raised to the war strength of 640 men. The corvettes have each about 250. These men are obtained from the Turkish coast provinces, in which naval service is now obligatory. The term of service is seven years, supplemented by five years in the Rediff or Reserve, which is now called out. The total number of seaman is stated at 50,000. The above may be considered a fair statement of the active portion of the Turkish Navy. Though small in numbers, it must be remembered that it is compact. The situation of the Turkish empire is such that this small, well-armed fleet will be able to hold its own in the Black Sea without any chance of the Russian squadron in those waters being able to obtain any reinforcements, as the Dardanelles may be said to hermetically seal the entrance.

In addition to its fighting ships, Turkey has a long list of vessels available for the transport of troops. She possesses three old wooden liners, mounting an aggregate of 254 old-fashioned guns; five wooden frigates and seven corvettes, with some 300 guns, besides twenty-one smaller craft with about eighty guns; all of these are screw-steamers. There are four paddle corvettes, mounting four guns each; three large sailing cruisers, with an aggregate of eight guns; and twenty-two avisos, or dispatch boats, carrying sixty-four guns in all. Of these last, however, a large number are very old, and hardly seaworthy. There are also three Royal yachts of great speed, which might be utilised as dispatch boats, and five old transports mounting two or three guns each. In addition to this, Turkey has twenty-nine large steamers belonging to companies or to private individuals, which would, no doubt, be made use of should Abdul Kerim think fit to carry out the idea of landing a Turkish force in the Caucasus, and calling upon the wild tribes there to revolt against the iron rule of the Czar. The Turkish fleet will have its work to do in the Black Sea, as Russia has spared neither trouble nor expense in its fortifications; but stone walls and torpedoes will be the only enemies it will have to contend with, as the Russian fleet is so manifestly its superior that it will never be able to meet it in fair fight.—*Iron.*